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NOTES AND COMMENTS.

The Bibliography of Zoology.

We publish in this number two items of considerable interest to zoologists. We have of late drawn attention to the efforts of various people to organise and improve the existing bibliographic service, and we have urged the need for unselfish co-operation and sinking of existing rivalries. We are, therefore, exceptionally pleased to be the means of announcing the fact that the zoologists of the University of Minnesota intend to devote their remarkable energy to the service of the proposed Central Bureau. The attempt which they were making was deserving of all praise; and had it met with support and success, such praise would have been unstintingly bestowed. When, therefore, Professor Nachtrieb and Mr. Clarke Barrows not merely withdraw from competition, but subordinate their services to an organisation which, though undoubtedly international in character, has been established by other individuals than themselves, they are, we consider, even more praiseworthy than they were before, and have given a better earnest of their good faith than even a million printed cards. Let us hope that other corporations and individuals will follow the admirable example of the vigorous newly-started venture of Minneapolis and of the old-established and solidly-based *Archiv für Naturgeschichte* in Berlin.

The proposed Central Bureau is also warmly supported by the Zoological Society of France, by the Naples Zoological Station, by almost all the zoologists of Belgium and Holland, including that eminent worker in the cause of scientific bibliography, Mr. Mourlon, the Director of the Belgian Academy of Science, as well as by national committees in Russia, Hungary, and many other countries. It is, therefore, unlikely that England will stand aloof. In the article that Dr. Field has written at our request, he has stated exactly what it is that is required of England, or let us say of the Zoological Society, which in this matter worthily represents England. But on

reading Dr. Field's statement, we are astonished to find that all the giving seems to be the other way. We are to have the *Zoological Record* completed and improved (and if anyone denies that it needs this, we hope he will no longer keep silence); we are to have its publication guaranteed and continued on the same lines as hitherto; our recorders and their editor are to be saved nine-tenths of their drudgery, and yet to be paid no less; and, in return for all this, the Zoological Society will actually be asked to spend less money than it loses at present. We have had opportunities of cross-examining Dr. Field, we are convinced of his good faith, and we see that he is in a position to bring his promises to performance.

A PALÆONTOLOGIST ON ZOOLOGICAL SYSTEM.

WE are pleased to be able to present our readers in this number, through the kindness of Professor Karl von Zittel, with a translation of the address which he delivered to the International Congress of Geologists last year. It is always interesting to hear the opinion of an acknowledged leader of his science on such matters of general importance, and Professor von Zittel's warning is not the least of the benefits arising from the recent assembly.

We are struck by the passage in which he compares the scrupulous anxiety that formerly accompanied the establishment of a new genus with the reckless abandonment in which genera are scattered abroad to-day. It is true that, when we regard the history of scientific nomenclature, we are unable to acquit even the older writers of too great indulgence in an irresponsible liberty; and it must be confessed, even by Professor von Zittel, that many of the difficulties under which we now labour are due to authors, who may, indeed, have been very careful to see that their genera were really new, but who did not take such pains as would ensure those new genera being understood by their successors. Nevertheless, it is just as well that our younger writers should have brought home to them a due sense of the extreme reponsibility that attaches to the publication of a new generic or specific name. A name once published is irrevocable; it may be right or wrong, but it can never be withdrawn; it remains a permanent addition to the labour of future investigators, and if wrong, then an irremovable stumbling-block.

The general question, phylogenetic research and the reconstruction of our classifications, though one of enormous importance, is so vast that we dare not enter upon it in a casual note. This only we would venture to say in extension and elucidation of the Professor's remarks. Darwinism and the Doctrine of Descent came upon the majority of scientific workers, especially on the describers and the systematists, almost too suddenly, and the momentum of its impact seems to have carried many of them off their feet. It was at once supposed that traces of descent were to be found everywhere by simple inspection. Hence a rashness in suggesting relationships on the

evidence of resemblances which, though striking at first sight, have been shown by more careful research to be due to convergence, homoplasy, or some other cause than affinity or descent. For instance, the stalked larva of *Antedon*, to which Professor von Zittel somewhat sarcastically refers, has actually been brought forward by writers of no mean reputation in confirmation of their peculiar views about certain Carboniferous crinoids, although slight reflection might have convinced them that no connection was possible. *Spirula*, as readers of Dr. Pelseneer's beautiful monograph in the "Challenger" Report may gather, has been another notorious instance. Just because it happens to be coiled, and to have a spherical initial chamber, it has been considered a connecting link with the ammonites, although, as Dr. Pelseneer shows, it is really descended from some ancestor of belemnites or cuttle-fish. It is against this foolhardiness of speculation, based on ignorance and an insufficient appreciation of the needs of the genealogist, that Professor von Zittel's protest should be raised. So far we are in full agreement with our learned leader; but we would not carry his caution too far. We believe in the reliability of palæontology and embryology, so long as they are kept within their proper limits, checked by one another and by the evidence of comparative anatomy and general ontogeny. We believe that palæontology especially is emerging from the mists of its first madness, and arriving, through sober study and minute comparisons, at results of validity and importance. Though every result may not at once be accepted, yet we cannot doubt that the methods employed by Hyatt, Cope, Jackson, and Beecher in America; by Branco, Würtenberger, Buckman, and a few others in Europe, are correct in principle, and we are glad to find that they meet with the approval of so eminent an authority as Von Zittel. The time may not have arrived for the reform of the entire zoological system; but that is no reason why all reform should be refused. Zoology is a growing science, and it must have its growing pains. It is only by the proposal of new classifications, and by the balancing of one against the other, that we shall be enabled to select the best in the end. Change is inevitable; and it is better that it should take place gradually, than that all the spars and tackle by which our voyage has been so far accomplished, should be kept till they are rotten, and then, on a sudden, go by the board.

Sir William Flower on the Principles of Nomenclature.

The subject discussed by Sir Henry Howorth in recent issues of Natural Science, and our comments thereon, have aroused no little interest. A correspondent calls our attention to an extract from part 2 of Sir William Flower's Catalogue of Osteology of Vertebrated Animals in the Museum of the Royal College of Surgeons. This was published in 1884, and as it seems to us of considerable importance, we print it in full:—

"In selecting the name chosen I have been mainly guided by the views which have been gradually gaining general currency among conscientious naturalists of all nations, and which were formulated in what is commonly called the 'Stricklandian Code,' adopted by a committee of the British Association for the Advancement of Science in 1842, and revised and reprinted by the Association in 1865 and again in 1878. These are nearly the same in principle as those concisely and clearly laid down by Isidore Geoffroy Saint-Hilaire in the introduction to his unfinished Catalogue of the Mammalia in the 'Muséum d'Histoire Naturelle de Paris' (1851), and those so copiously elaborated in Mr. Dall's Report of the Committee on Zoological Nomenclature to the American Association for the Advancement of Science at the Nashville meeting in 1877. The regulations laid down in these codes for the formation of new names are unimpeachable; and although some of the rules for the selection of names already in existence have given rise to criticism, and are occasionally difficult of practical application when an endeavour is made to enforce them rigidly, they do in the main, when interpreted with discretion and common sense, lead to satisfactory results. As what we are aiming at is simply convenience and general accord, and not abstract justice or truth, there are cases in which the rigid law of priority, even if it can be ascertained, requires qualification, as it is certainly not advisable to revive an obsolete or almost unknown name at the expense of one which, if not strictly legitimate, has been universally accepted and become thoroughly incorporated in zoological and anatomical literature; and it is often better to put up with a small error or inconvenience in an existing name than to incur the much larger confusion caused by the introduction of a new one.

"Of all the various groups into which animals are conventionally divided by zoologists, such as classes, orders, families, genera, species, etc., the last two are of greater importance than any of the others, as upon the limits assigned to them the name of the animal depends. It matters comparatively little how we arrange and rearrange orders and suborders, families and subfamilies in our endeavours to express our views of the affinities of their members to each other; but directly we apply the same process to genera and species we begin to introduce that greatest source of trouble and perplexity to students, and most fertile source of impediment to the progress of zoological knowledge, the multiplication of names of the same object. All zoologists seem to be agreed as to the value of the system introduced by Linnæus, by which the name of the animal is determined by the genus and species under which it is placed, and all attempts to improve and modify this method of nomenclature have ended in failure. It might have been supposed that this general agreement would have preserved these groups, especially the former, from the inconsiderate, hasty, and useless alterations to which they have been incessantly subjected by zoologists who have often contributed nothing else to the development of their science. I do not mean that with the advancement of knowledge improvements cannot be continually made in the current arrangement of genera. The older groups become so unwieldy by the discovery of new species belonging to them that they must be broken up, if only for the sake of convenience; newly discovered forms which cannot be placed in any of the established genera must have new genera constituted for them, and fuller knowledge of the structure of an animal may necessitate its removal from one genus into another: all these are incidents in the legitimate progress of science. Such alterations, however, should never be

made lightly and without a full sense of responsibility for the difficulties which may be occasioned by them, and which often can never be removed. Complete agreement upon this subject can never be expected, as the idea of a *genus*, of an assemblage of animals to which a common generic name may be attached, cannot be defined in words, and only exists in the imagination of the different persons making use of the expression; but there might be no difficulty in coming to some general agreement, if individual zoologists would look at the idea as held by the majority, and would not give way to the impulse to bestow a name wherever there is the slightest opening for doing so. In the following Catalogue not a single new division has been proposed, or a new name introduced; but, on the contrary, very many of the generic divisions of modern zoological writers, founded upon most trifling characters, often artificial or even erroneous, have been ignored, as it is thought that the sooner such names are discarded the better. Others have certainly been admitted which, according to my judgment, it would have been better never to have invented; but as they exist and are generally recognised, less confusion and alteration of existing nomenclature is caused by retaining than by abolishing them.

"Subgenera with names attached to them have always been avoided, as they cause confusion of nomenclature, and nearly always end sooner or later in being promoted to the rank of true genera. It seems preferable in the case of large genera, showing much diversity of characters among their members, to group together those which resemble each other most into sections, but avoiding the use of any distinctive name that would clash with the binomial principle."

American Plant Nomenclature.

In the March number of the *Botanical Gazette* Dr. Robinson severely criticises the "List of Pteridophyta and Spermatophyta of North-Eastern America," which has just been prepared by the Nomenclature-Committee of the Botanical Club. As the expression of the latest phase of nomenclature-reform in the New World, the list affords ample material for a discussion of the merits or demerits of a system, and, by the way, useful object-lessons for the systematic botanist as well as warning to the "nomenclaturist." The chief point raised by the writer is the stability of the new system. Does it possess the elements of permanency? Is it really a rigid code, allowing no exceptions and leaving nothing to individual judgment? It has none of these merits. On the contrary, as Dr. Robinson states, every working botanist knows that the selection of the first specific name, after the still more difficult choice of the generic, often involves most critical judgment both as regards the exact application of brief and unsatisfactory descriptions and the often doubtful priority of publications. A striking illustration that even the form of the name may be the subject of individual judgment or arbitrary modification is adduced from the "List," in which the name adopted by Professor Britton for the pretty little garden plant, Sweet Alyssum, is shown to be founded on a supposition entirely unwarranted by fact. Strangely enough, the name which Professor Britton has evolved and

claimed as his own turns out after all to be identical with that given for a different reason to the same plant by no less a person than Robert Brown. Was Robert Brown's combination overlooked? or is also the motive for the name to be taken in consideration when adjudging the right of authorship?

Again, the system of renaming *both* plants when species of the same specific name are brought together from the readjustment or union of genera, instead of conducing to stability, gives perpetual opportunity for change. One shudders at the contemplation of the results of the union of *Aster* and *Erigeron*, *Panicum* and *Paspalum*, and the consequent displacement of names which, on the principle of "once a synonym always a synonym," can never be revived. The misuse of the trinomial system, lately condemned in NATURAL SCIENCE, is also criticised by Dr. Robinson, with the remark that "a style of nomenclature in which there is no distinction between subspecific, varietal, and formal differences is likely to appear to future botanists a rather clumsy tool." Once again we are reminded that ready intelligibility is a more important quality than stability and consistency, or, at all events, the degree of stability and consistency attainable.

There is another argument, to which Dr. Robinson does not allude, against the replacement of well-known names by strange combinations. Many of the former have their origin in a valuable monograph of a group on which some painstaking worker has spent much time and thought, and his duly accredited specific names inspire a certain respect and confidence which the strange combinations of the exponent of a system of nomenclature can never claim.

THE PUBLICATION OF PAPERS BY SOCIETIES.

THE point raised by Mr. Cockerell, in the letter that we print in this number, is one of considerable importance to scientific writers, and has a wider bearing than the writer seems to be aware of. We are authorised to state, if need be, that Mr. Cockerell is in no way biassed against the Zoological Society, and that he would not be thought to be making an attack on its learned and courteous secretary. This, however, is an almost unnecessary statement, for the complaint is one that may be brought against nearly every learned society that is in the habit of publishing the contributions of its members. It makes but little difference whether a society has a publication committee, or a single responsible editor, or whether it calls in the aid of a referee. In almost every case, as affairs are at present conducted, an author's paper is first sent in to the secretary, then read in open meeting and discussed, then (it is probable) made still more public by some printed abstract, which is theoretically rather than practically issued for the private convenience of the fellows alone. After the author's hopes have thus been raised; after

his results have been proclaimed to contemporary, perhaps to competing, workers; after a lapse of time, which in the case of some work may be fatal to the author's claim of priority: it may be that, after all this, his paper is refused ultimate publication. The refusal to publish may be based on valid grounds; we have sufficient confidence in the common sense and in the integrity of scientific men to suppose that such is usually the case. But there are cases notorious in the history of science, and there are cases that one hears of every year, if not every day, in which a paper is rejected by a society, either because its views do not jump with the personal views of the referee, because the author is not altogether a *persona grata*, or because some merely technical condition has not been observed. Mr. Cockerell's case is none of these: the paper appears to have been declined simply because the Zoological Society did not think it its business to descend to the publication of the description of so insignificant a creature as a new Coccid; and the Society has a perfect right to limit its own sphere of action. But in this case, as in all the others, the delay and the semi-publication are a hardship, though not necessarily an injustice to the author.

It may be replied to this complaint that an author sends his paper to a society, just as he sends it to the editor of a magazine, under certain conditions that are (or that ought to be) well known and clearly defined. Some societies very properly send a notice to every author, immediately on receipt of his paper, acquainting him with those conditions, and the author can then withdraw his paper if he choose. All this is very true, but the case is not thereby improved. There is a very considerable difference between the action of one of these learned societies and that of a competent magazine-editor. A society, we are told, keeps a paper more than two-and-a-half months; an editor, if he is going to decline an article, usually does so within as many weeks: some editors have been known to return articles within as many hours. Would it not, then, be possible for the procedure of societies to be approximated to editorial methods? These lengthy delays have a quaint, old-world, respectability about them, but they are scarcely suited to modern needs. Even the society must be a loser sometimes, quite as much as the author, especially when, as we know very well to be no rare occurrence, an author's paper gets into type before it is rejected. Might not a committee consider papers before they are read in public instead of after? It is no hard matter to decide whether a paper is or is not simple rubbish, or whether it comes or does not come within the scope of any particular society. As for any fear of unwarranted boycotting, or of illegitimate suppression, or of stealing an author's results, we know, to speak quite candidly, that these things have occurred quite often enough under the present system; and even if there were any greater temptations under the proposed alternative, at all events the author would no longer be handicapped by a two or

three months' delay. The acceptance of a paper need not, of course, prevent a little fatherly care at a later stage; some editorial pruning is generally beneficial to even the best authors. But as for the author's opinions, they should be left to him alone; for does not every society plainly advertise on its title-page that it is in no way responsible for the opinions of the authors?

TOXINS AND ANTI-TOXINS.

A TOPIC of the day is the treatment and prevention of diseases due to organisms, by what are known as anti-toxins. So much confusion exists in the popular writings on these subjects, that a brief statement of their nature may interest our readers.

In the early days of bacteriology it was thought that the damage caused by microbes was a direct, almost a mechanical damage. The microbes of various diseases (men said) lived and multiplied in the tissues, as external parasites may live and multiply on the skin or in the hair. Sometimes their presence caused irritation, and this, when it became severe, was held to give rise to inflammatory symptoms. Sometimes they caused mechanical lesions, actually destroying tissues, as the moulds of many skin-diseases destroy the epidermis. Most often they were held to play their disagreeable part by occluding blood-vessels or lymphatics, by blocking up natural passages or by producing mechanical pressure on nerves. No doubt all these processes are accountable for many of the symptoms of diseases; but it is becoming more and more clear that they are not the most serious factors.

Many years ago Dr. Burdon Sanderson insisted upon the living nature of these organisms, and on the damage they caused as being, in a sense, the result of the conflicting vitalities of the microbes and the tissues. The Toxin theory of diseases really is an amplification of this point of view. A microbe living in a tissue exhibits vital phenomena peculiar to its nature, just as a larger organism differs from its fellow-organisms of other species and genera. As necessary bye-products of their metabolism, many micro-organisms discharge chemical substances into the surrounding media. These, for the most part, are complicated proteid substances, akin to the globulins and paraglobulins. Some of them, passing into the blood and being carried from tissue to tissue, go their round harmlessly, and are extruded by the skin or by the kidneys as useless or waste material. Others, and such are the discharges of the microbes of dangerous diseases, are known as toxins. Being carried through the body by the blood, they exhibit an unholy affinity for some of the cells of the body—generally for some of the nerve-centres. Entering these, they disarrange or even destroy the normal procedure of the cells, with the result that these poisoned cells are unable to discharge their peculiar functions. And so, from a nidus of bacteria growing, say in a toe, there may be discharged a poison that is carried by the blood all

over the body, doing no damage save to the nerve-centre controlling, say, the mechanism of respiration.

In some cases the toxins have been prepared from cultures of the microbe on gelatine or agar-agar, and these toxins, carefully separated from the parent-bacteria, cause the dangerous symptoms of the disease, when they are introduced directly into the blood.

The anti-toxins are more difficult to understand. It seems that, as the toxins are discharged into the blood, the blood "makes an effort" to neutralise them by producing a counteracting proteid substance. In some animals it appears as if the anti-toxin for a particular toxin was present naturally, and such animals would naturally be immune to the ravages of the microbe in question. But in all animals, if the toxin be supplied slowly, there is an effort made to manufacture the counteracting agent. The secret of anti-toxin treatment is to select an animal constitutionally able to manufacture the anti-toxin of any particular toxin; to stimulate the production by gradually increasing doses of the toxin, and then when the blood is filled by the anti-toxin to remove and isolate the latter. When the anti-toxin, so prepared, is injected into the blood of a patient suffering from the disease, it neutralises the toxins in process of being discharged; and when the most dangerous symptoms have been mitigated, no doubt the tissues are enabled to manufacture the necessary further supply of anti-toxin themselves.

Such is the general theory of toxins and anti-toxins; in each particular case there is still needed experience and experiment.

The Sexual Organs of Ferns.

Mr. D. H. Campbell communicates to the *Botanical Gazette* (of February) a note on the origin of the sexual organs in Ferns. A study of the development of *Marattia* led the author to conclude that this genus and its nearest allies had arisen from forms resembling *Anthoceros*, a somewhat peculiar genus of Liverwort. The sexual organs in *Anthoceros* differ from those generally characteristic of Mosses and Liverworts, in that they are buried in the tissue of the plant-body and are not protruded and borne on stalks. The female organ, in particular, is of more simple structure, recalling that of *Marattia*, and, to a less extent, of the more typical ferns. The author is convinced that, in tracing the pedigree of the Fern group as a whole, we must work back through forms like *Marattia* to a moss-ancestor resembling *Anthoceros*.

Botanical Education in America.

In the same number, the editor strikes a note of warning *à propos* of methods of botanical instruction prevalent in American colleges, not, he says, those "in which botany receives but little attention," but "those in which there is an attempt to develop it in a full and

modern way." "In the recoil from the old-time methods, the college has gone to the other extreme and seeks to become a place of research." "Raw young men and women, after a year or two of the study of 'types,' are assigned original problems, and their uncertain results, with more or less revision on the part of the instructor, are published in some periodical or bulletin." These remarks are worth the attention of all teachers of the science, whose first aim should be to give their students a thorough groundwork in general principles, suppressing, if need be, undue haste towards detailed specialising in any one direction. A man or woman with a good general knowledge will make, for instance, a better systematist than one who has acquired, at the expense of the former, detailed information in one group of plants.

The science of botany is too vast for a year or two's work to be sufficient preparation for creditable research in any one branch. "The different departments of it are so interdependent that it needs long training to bring the perspective and the grasp that make any independent investigation profitable."

The warning, of course, applies to other Sciences besides Botany, and may not be wholly unneeded in our own country.

MR. WALLACE ON ORGANIC EVOLUTION.

IN the *Fortnightly Review* for February and March, Mr. A. R. Wallace deals with the views of Bateson and Galton. His essays form a closely-reasoned and interesting statement of the case for Natural Selection as against positions of organic stability. He urges that Mr. Bateson has not been directing attention to a class of facts hitherto neglected by Darwinians. Darwin himself considered with great care the kind of variations that Bateson calls discontinuous, and came to the conclusion that they had little or no effect on the origin of species. For the most part they are sports or abnormalities, and grade into monstrosities. Mr. Wallace points out that Mr. Bateson is quite wrong in saying that discontinuous variations are similar to the characters used to define species and genera. Systematists know well that "among the higher animals, at all events, it is not so." As a matter of fact, specific characters, and, indeed, generic characters, in the vast majority of cases, both among higher and lower animals, are of a kind similar to individual and continuous variations.

Galton and Bateson have "devoted themselves too exclusively to one set of factors, while overlooking others which are both more general and more fundamental. These are the enormously rapid multiplication of all organisms during more favourable periods, and the consequent weeding out of all but the fittest in what must be on the whole stationary populations. And acting in combination with this *annual* destruction of the less fit is the *periodical* elimination under recurrent unfavourable conditions of such a large proportion of

each species as to leave only a small fraction—the very elect of the elect—to continue the race."

Specific Stability.

At a recent meeting of the Entomological Society (see *Nature*, April 11, 1895) Mr. F. Galton appealed to those who had made experiments in breeding, for information on three points connected with his theory of positions of Specific Stability. First, he wishes to be made acquainted with "instances of such strongly-marked peculiarities, whether in form, in colour, or in habit, as have occasionally appeared in a single or in a few individuals among a brood; but no record is wanted of monstrosities, or of such other characteristics as are clearly inconsistent with health and vigour." With due deference to the eminent inquirer, may we suggest that the distinction between monstrosities and discontinuous variations is one demanding more interpretation than is congruous with the mere collecting of facts. How is it to be decided whether or no a particular variation be a monstrosity? Moreover, if positions of organic stability exist, it would seem to us natural that monstrosities should exhibit signs of them as much as what Mr. Galton might consider not to be monstrosities. If a monstrosity be capable of breeding, the record of its descendants would afford most valuable indications of the range and character of heredity. Whether or no a particular variation were inconsistent with health and vigour, in many cases, must depend on the environment. On the protection theory of coloration, a caterpillar, varying from the protective pattern of its kind, would be a monster doomed to perish: under the conditions of the breeder, or in the natural absence of enemies, would it propagate its unnatural variation?

Secondly, Mr. Galton asks for "instances in which any of the above peculiarities has appeared in the broods of different parents." This point is the root of the whole matter. If such positions of organic stability do occur, they must occur repeatedly. In fact, in addition to each species, there must be a number of possible "para-species" (Mr. Bateson, who is a purist, will object to the term).

To take a theoretical example of "para-species." A species of butterflies with a particular coloration may range over a plain on which occur isolated high mountains. On each of these high mountains may occur the same "arctic variety" of the species. On the theory of positions of organic stability these arctic-forms may be identical and yet of independent origin on each mountain. They are the outcome of a "position of organic stability" near to the plain-form of the species. They (on the theory) have been called into existence, or have been selected by the conditions on the mountain-tops; they are "para-species" of the plain-species. Such a condition of things would simplify one's conception of the process of organic evolution. At present it is pure theory, but if Mr. Galton is successful

in getting instances of the natural occurrence of such "para-species" born of different parents, it will go far towards establishing the point.

Mr. Galton's third request is for "instances in which any of these peculiarly characterised individuals have transmitted their peculiarities, hereditarily, to one or more generations," and he asks special attention to be directed to the degree of intensity with which the peculiarity has been transmitted, to the occurrence of any dilution of the peculiarity when transmitted, or to the absence of its transmission. The point of the last inquiry is clear enough. If positions of organic stability are real things, frequently enough they should be transmitted as readily and as fully as that position of organic stability known as the normal species.

A Burlesque Explanation.

Many things have been written about Natural Selection that will last not even our own day; but a Mr. S. E. Peal seems to us to have reached a climax of illusory ingenuity. We regret that Mr. Alfred R. Wallace has laid himself open to the enemy by formally supporting Mr. Peal in *Nature*. It seems that certain tan-spots occur over the eyes of semi-domesticated dogs. These do not exist in wild animals allied to the dog, or in the modern breeds of fully-domesticated dogs. The spots are most conspicuous when the eyes are closed, appearing then like opened eyes. The ingenious suggestion is that they "may have been protective to the animals during sleep, causing them to look as if awake. The reason that they do not occur in wild dogs may be that the latter conceal themselves when sleeping, which the half-domesticated animals were not able to do."

To Mr. Wallace, whose words these are, we must point out with the greatest deference that animals in a state of semi-domestication would hardly need so special a method of protection. No doubt the ways of the young savage might not be agreeable to the dog, and it would be to the advantage of the dog to appear wide-awake; but even the young savage would sleep sometimes, and probably the semi-domesticated dog might snatch enough sleep o'nights to maintain his existence. But it is not against the particular case, but against the general application of "Natural Selection" to individual features that we wish to protest. To argue about this individual feature we should have to know all about the dog and his structure and habits. With what other changes are these particular spots associated? The mere proposition of the question is enough to dispel at once any idea of accepting Mr. Peal's suggestion. For each individual feature a dozen ingenious theories might be formed; but one and all the theories must be illusory in the absence of any knowledge as to the correlations that exist in the bodies of the organisms in question.

Curiosities of Mollusca.

We hope to give a detailed review of the "Cambridge Natural History" in our next issue; but there are so many interesting tit-bits of information in the volume which has just appeared that we feel justified in anticipating a little. Everybody had heard of the use of the cowry shell as money. But, possibly, the extent to which this is legal tender is not so well-known. Mr. Cooke relates how a gentleman in India paid for a house, which he had erected, entirely in these shells. The house cost him £400; the equivalent of this sum in shells was 16,000,000, which were paid as such. Not only is this particular shell used as specie; in other parts of the world other shells are put to the same use. The wampum of the American-Indian is simply an elaborate ornament formed of shells of the common clam, *Venus mercenaria*; an Indian, therefore, like a Belgian maiden with her gold ornaments, carried his fortune upon his person.

But shells have a value also among civilised races. They are apt to be dear to the collector; and rare species have fetched large prices. Mr. Cooke has brought together some excerpts from auctioneers' catalogues, from which it appears that £100 is the record price asked for a shell. The specimen which was supposed to have this extraordinary value is one of *Pleurotomaria adansoniana;* but it has been sold to the British Museum for about half that sum.

Mr. Cooke has brought together some interesting cases of supposed mimicry among the Mollusca, of which the following is perhaps the most striking. "Certain species of *Strombus* (*mauritianus*, L., *luhuanus*, L.) show a remarkable similarity in the shape of the shell to that of *Conus*, so much so, that a tyro would be sure to mistake them, at first sight, for Cones. In the case of *S. luhuanus* at least, this similarity is increased by the possession of a remarkably stout brown epidermis. Now *Conus* is a flesh-eating genus, armed with very powerful teeth which are capable of inflicting even on man a poisonous and sometimes fatal wound. *Strombus*, on the other hand, is probably frugivorous (*sic*), and is furnished with weak and inoffensive teeth. It is possible that this resemblance is a case of 'mimicry.' It is quite conceivable that powerful fishes which would swallow a *Strombus* whole and not suffer for it, might acquire a distaste for a Cone which was capable of lacerating their insides after being swallowed. And, therefore, the more like a Cone the *Strombus* became, the better chance it would have of being passed over as an ineligible article of food."

"Protective coloration" is not unknown among molluscs. The *Scyllæa* of the gulf-weed, with its olive-brown hues mixed with white spots, is one of the most familiar instances. Mr. Cooke draws attention to the fact that the "Blue Limpet," *Helcion pellucidum*, which, when young, has beautiful iridescent tints, lives at that period of its life exclusively upon the fronds of the great *Laminaria*, which has also iridescent hues. Later in life, when the iridescence fades,

it transfers its abode to the lower part of the "stalk," and eventually to the "roots" of the Sea-weed, which are not iridescent.

THE QUEEN'S OSTRICH.

THERE has been a death-rate of forty per cent. among the ostriches living in the gardens of the Zoological Society during the last few weeks. To leave the delusions of statistics, that means that two out of five ostriches have died. One of the deaths was that of a very curious piebald male ostrich, the property of Mr. Rothschild. The feathers of that fine bird were all black and white, so that the general aspect of the creature was most curiously mottled. It is being prepared for the Tring Museum, so that naturalists may still have an opportunity of studying this most remarkable variation.

The other death was that of the large male, presented to the Society by the Queen some time ago. It was an unusually large and fine bird, the length of the bones of the leg from the head of the femur to the toe being over seven feet. A few months ago it was in magnificent condition, but during its illness the feathers became so broken and draggled that the skin was useless. The bird was ill for several weeks, gradually becoming weaker. For a number of days before its death it refused to eat.

A correspondent who happened to be present while the examination of the body was being made, informs us that the only organs obviously diseased were the lungs. These showed signs of severe broncho-pneumonia. At the *post-mortem* on an ostrich, the chief excitement is the examination of the contents of the gizzard. Usually there is a remarkable assortment of curiosities—glass bottles, coins, and all manner of foreign bodies. The Queen's ostrich apparently was a fastidious bird. Its stomach contained nothing but grass and pebbles.

CARICATURES IN TEXT-BOOKS.

THE cheapness of electrotypes is not an altogether unmixed blessing. When a book-illustration becomes popular, it is reproduced over and over again; and even if it happens to be proved erroneous, an incalculable period is required to eradicate it from the most approved treatises. Figures of vertebrate fossils are particularly unfortunate in this respect. The Nestor of American geologists, the sad news of whose death arrives as these pages go to press, had just issued a fourth edition of his classic "Manual of Geology," and even here there are some woeful relics of antiquity. Professor Dana's book is admirably up-to-date in most matters, and it is refreshing to see the multitude of new illustrations introduced; but we may be pardoned for exclaiming when we find among the figures of fossil vertebrata so many defunct friends which we had hoped were already decently buried. The Devonian *Coccosteus* restored as if it were a

modern bony fish; *Dipterus* still borrowing the head from a codfish; *Dapedius* and *Aspidorhynchus* bearing even less likeness to the originals than the once-popular silhouettes—all these greet us again. "*Pterodactylus crassirostris*," with four free fingers on the hand, appears as if it would never die. It would at least have been possible to cut off the superfluous digit, as is now done by those who still use the block in Europe. Cope's original guess at the form of the body in the Chalk fish *Portheus* (reproduced on p. 844) was long ago corrected; and of this, as of the other animals mentioned, adequate illustrations could have been found in the literature of the last two decades.

The Land and Water Areas of the Globe.

Professor Hermann Wagner has an interesting paper on this subject in the *Scottish Geographical Magazine* for April. He has worked out a new calculation by zones of ten degrees of latitude. The figures are so divergent from Dr. Murray's areas (*Scott. Geogr. Mag.*, ii., 1886, p. 553) that Professor Wagner has sought for some confirmation of the new results. This he has found in the data of a new calculation of the mean depths of the oceans which has been published by Dr. Karl Karstens ("Neue Berechnung der mittleren Tiefe der Ozeane," Kiel, 1894), who divides the twenty-seven seas of the globe into trapezes of one, two, or three degrees in breadth. Adding these areas together, the surface of the land can be indirectly deduced. The agreement obtained is astonishing, for while Professor Wagner by direct calculation arrives at 45,417,000 square miles as his estimate of land area, Dr. Karstens' data give him by indirect calculation 45,445,000 square miles. Professor Wagner considers his figures the more reliable. Only the zones between 60° N. and 60° S. allow of exact comparison, for Dr. Karstens takes the Polar limit at 66½°.

Professor Wagner is of the opinion that some 250,000 square miles will include all the land in the Arctic regions, while he agrees with Dr. Murray that 3,500,000 square miles is a likely figure for the Antarctic lands. The proportion of land to water works out at 1 : 2·54. Professor Wagner promises an exhaustive paper in the *Beiträge zur Geophysik*, vol. ii., no. 3, shortly to appear at Strasbourg.

Wild Fowl in Norfolk.

Under the Wild Birds' Protection Act, 1894, the Home Secretary has recently issued an order for the special preservation of the birds of Norfolk. Frequenters of the Broads and of the coast of Norfolk, or readers of the attractive book by Mr. P. H. Emerson, "Birds, Beasts, and Fishes of the Norfolk Broad-land," which has just been sent us from Mr. D. Nutt in the Strand, will be aware that many birds, rare elsewhere in our islands, are still to be found in some of these secluded haunts, though the extinction of several is threatened.

The order forbids the taking or destroying of the eggs of any kind of wild birds within specified areas for one year, and makes a general prohibition for the entire county in favour of the following birds:—The Bearded Titmouse or Reed Pheasant; the Crossbill; the White or Barn Owl; Wild Ducks and Teal of all species; the Norfolk Plover, Stone Curlew or Thick-knee; Ruff or Reeve; the Ring Dotterel, Ring Plover or Stone Runner; Oyster Catcher or Sea-pie; the Terns, Sea Swallows, Pearls or Dip-ears, all species; the Great Crested Grebe or Loon. Other birds there are whose names might well be added to this list; we may mention the Bittern or "Buttle," as he is called, the Spotted Rail, the Water Rail, and the stately Avocet.

THE ANATOMY OF ORNITHORYNCHUS.

IN the newly-issued number of the *Proceedings* of the Zoological Society of London (part iv., 1894), there are two important contributions to our knowledge of the duck-billed Platypus. Mr. Manners-Smith has gone over the muscular system carefully, correcting his homologies by examination of the nervous supply. The details are too technical to be entered upon here, but they raise the very interesting question of the value of muscles from the point of view of systematic zoology. It is not too much to say that there are only two opinions on this matter. Those who have worked at the muscles of fishes, reptiles, birds, or mammals believe them to be almost as valuable as bones or nerves, though much more difficult to interpret. Those who have not worked at them tend to despise their value. No doubt while muscles were identified in a rather cavalier fashion, and when the degeneration of muscles into ligaments was not understood, there was little hope of getting valuable results. In spite of the vast amount of work already published, we take muscular anatomy to be one of the subjects of the future.

Mr. F. E. Beddard deals with several interesting points in the viscera. The first is the presence of a ventral mesentery. It is well known that in the Dipnoi and the Amphibia the gut is suspended to the ventral wall of the abdomen by a mesentery. In the frog, this is conspicuous by reason of the anterior abdominal vein, which the elementary student cuts through with a monotonous regularity. In mammals, a short but similarly-placed ventral mesentery exists in the region of the liver, and is known as the falciform ligament. Balfour, in his embryology, cast doubt on the obvious comparison between these two structures; but recent investigations of Professor Howes on the Australian *Torpedo*, and of Mr. Beddard, on *Ornithorhynchus* and *Echidna*, make it seem probable that the falciform ligament is a remnant of the primitive ventral mesentery. Some time ago, Mr. Beddard found that there was an anterior abdominal vein in *Echidna*; he has now found that there is a well-marked ventral mesentery, although it has no vein, in *Ornithorhynchus*.

I.

Palæontology and the Biogenetic Law.[1]

PALÆONTOLOGY has long ceased to place itself exclusively at the service of geology as the study of characteristic fossils. It has gradually grown into an independent branch of the biological sciences, and claims a share in all their movements and tendencies. The conclusive establishment of the Doctrine of Descent has evoked the most powerful revolution in descriptive natural history, influencing and transforming its whole method of research. No large palæontological work of to-day contents itself with the description of new forms, the comparison of them with those already known, and the arrangement of them in systematic order. To determine the genetic relationships, the ancestry, the modification, and the further development, in short, the race-history or phylogeny of the organisms under consideration, is now regarded as essential, by many indeed as the chief aim of palæontology.

As Darwin forcibly insisted, the doctrine of descent depends in no small degree on palæontological facts. Thus, the great similarity of the fossils occurring in strata immediately superposed one on another, *e.g.*, the brachiopods, ammonites, and other molluscs, has made it difficult for geologists to determine the age of sedimentary rocks. In recent years a great number of closely-allied species have been traced through several superposed beds, stages, or divisions of formations, their exact morphological relationships have been studied in the most careful manner, and thus the probability at least has been established, that we are here dealing with a genealogical sequence of blood-relations. To be sure these do not as a rule form complete chains, wherein mutation is linked with mutation and species with species. They are rather discontinuous series, of which all the members change in a definite direction, and obviously form steps in a line of development, which culminates in the last extinct or still-existing representatives. Among the better-known series of forms I will only refer to a few. The succession of genera, which leads from *Hyracotherium*—perhaps, indeed, from the five-toed *Phenacodus*—through *Paloplotherium*, *Anchilophus*, *Anchitherium*, and *Hipparion*, to the single-toed horse, forms one of the most quoted and most beautiful examples

[1] A paper read before the International Congress of Geologists, 1894.

of phylogenetic development. No less complete a series is presented by the genealogical tree of the camels, which already appear during the Eocene in North America, spread there in the Miocene and Pliocene, and then first emigrate to the Old World. The pigs also, and the Oreodontidæ, Anoplotheriidæ, Tragulidæ, and the ruminants studied in so masterly a manner by Rütimeyer, afford us more or less complete genealogical series, beside which may be ranged the crocodiles among reptiles, the Amioidei and Physostomi among fishes. If we glance over these phylogenetic series, we observe that the final terms are almost always distinguished from their predecessors by a more pronounced and distinctive differentiation; and since we are accustomed to assign a higher rank to a specialised organism in which every function is performed by a special arrangement, than to a creature which performs its functions with few and less complicated parts, phylogenetic development, as a rule, implies also progression and perfection. For the existence of the chains of forms already mentioned, to which many others may be added, there is only one rational explanation, namely, that the various links in them have arisen one from another, and are connected by blood-relationship. Moreover, the similarity of the faunas and floras which are nearest to one another in geological age, as well as the geographical distribution of extinct and still existing plants and animals, can only be satisfactorily explained on the assumption of descent.

But, although an abundance of palæontological facts can be cited in the most convincing manner in favour of the theory of descent, on the other hand we must not forget that we still know no point of origin for numerous independently arising creatures, and that the connection between the larger divisions of the animal and vegetable kingdoms is by no means so intimate as the theory specially postulates. The jubilation with which the discovery of *Archæopteryx* was greeted at the time shows at best that links had previously been wanting between two classes which among vertebrates undoubtedly exhibited the closest relationships. Further, *Archæopteryx* fills the gap between birds and reptiles only in a very imperfect manner, and affords no indication of the point at which the former have branched from the latter. It may, indeed, be maintained that we find ourselves to-day in greater uncertainty as to the origin of birds than we were twenty-five years ago, when Huxley's brilliant researches on the pelvis of the Dinosauria seemed to have found the bridge between the two classes. Links between the Amphibia and Reptilia are also still wanting. Perhaps they are to be found among the varied Theromorpha, but as yet palæontology cannot determine the phylogenetic modification of the amphibian into the reptilian type. No zoologist will deny that the Mammalia hold an entirely isolated position, separated by a wide gulf from birds, reptiles, amphibians, and fishes; while among all known mammals it is not some old fossil genus, but the duck-bill, still living in Tasmania, which most reminds

one of the more lowly organised vertebrates. Certainly we still know too little about the skeleton of the Mesozoic mammals, and especially the Allotheria, for us to form a final opinion on this point. But the warmest adherents of the theory of descent must at all events admit that extinct links between the different Classes and Orders of the vegetable and animal kingdoms are forthcoming only in a small and ever-diminishing number.

Nevertheless, in the larger groups we know numerous series of forms, which not only bear witness to the great plasticity and adaptibility of their members, but also in their chronological order indicate the line along which modification has taken place in course of time. To be sure, much uncertainty and the personal equation of the authors attach to those genealogical trees that are based entirely on the morphological comparison and determination of the chronological sequence of the forms met with. "It is easy to accumulate probabilities, hard to make out some particular case in such a way that it will stand rigorous criticism," was Huxley's caution so long ago as the year 1870, in his classic address to the Geological Society of London; and one of the most spirited veterans in the field of mammalian palæontology decides in his last exhaustive monograph on the fauna of Egerkingen, that the creaking and crackling of leaves and branches already decayed does not encourage one to set foot in the hastily-explored forests of phylogenetic trees. None the less does the tracing of the hidden bonds of relationship exercise a fascinating charm over every investigator. All of us, indeed, are convinced that the mutual relationships of the extinct and still-living members of any large group of organisms may be represented, not in the form of an entangled network, but in that of a much-branched tree.

In addition to the above facts there is still another series of phenomena which confirms the genetic connection of the palæontological chains of forms, and this was first observed, strangely enough, by one of the most distinguished opponents of the theory of descent. Louis Agassiz certainly regarded the fossil embryonic types as creative attempts which prophetically foreshadowed genera that appeared later with more mature characters. Fossil creatures with persistent youthful and even embryonic characters could not fail also to be noticed by the adherents of the theory of descent, but were regarded as favouring a view which recurred in very different forms in the philosophical literature of the first decades of this century, and which has lately been precisely formulated by our great German zoologist, Ernst Haeckel, under the name of the "Biogenetic Law." According to this, the development or ontogeny of each individual is only a short recapitulation of the long course of ancestral history (phylogeny) of the species or family in question. There must, therefore, also be chronological series of fossil embryonic types which would correspond with the different stages in the development of a subsequently existing form; indeed, the separate divisions of a

genealogical tree must correspond essentially with the ontogenetic stages of a determined course of development. If the biogenetic law were correct, embryology would thus be in a position to reconstruct, at least approximately, the primitive forerunners of each group of plants and animals; and these types should, if they were capable of preservation, also lie buried in the rocks.

If we consult palæontology, it shows that these surmises are by no means confirmed. There are, indeed, a great number of fossil genera which retain throughout life the embryonic, or, rather, the youthful characters of their existing allies, but it is only among the mammals, and to some extent among the reptiles, that I could name a complete series of forms following one another in time and belonging to the same line of development. The Eocene, Oligocene, and, in part also, even the Miocene Mammalia, stand to their now existing allies, for the most part, in the relation of youthful forms, while they, almost without exception, exhibit at least some characters which are quickly passed through by their geologically younger successors in the embryonic or youthful stage. On the other hand, they are, as a rule, destitute of the most striking peculiarities, such as antlers, bony processes, fusion of certain bones, reduction of the teeth or of separate parts of the skeleton; and it is not till we study more closely a series of related genera of different geological ages that we see how the differentiations and peculiarities of the existing representatives of any particular group have been gradually formed in course of time. But thus it is also possible to discover, in most of the mammalian orders, a number of primitive characters, which, while they frequently occur united in the oldest representatives of the group in question, also usually correspond to an embryonic stage of one of its living members.

The ontogeny of organisms now living would, for the rest, afford but an exceedingly unsafe basis for the reconstruction of ancient faunas and floras, since experience teaches that the biogenetic law is frequently veiled or completely obscured owing to various causes. Not seldom does it happen that, of two nearly allied living forms, the one passes through a series of continuous, successive stages, while development in the other takes place more by jumps. In the latter case the embryo is driven by peculiar influences to an acceleration of its development; it completely jumps over certain stages, and thus renders unintelligible the historical (palingenetic) record preserved in the ontogeny of each individual. This falsification of development—or Cœnogenesis, as Haeckel calls it—chiefly occurs when the adult individual manifests a high degree of differentiation, and when the embryo has to pass through considerable changes to reach its final form. How unsafe and deceptive palæontological results would be if attained by embryological paths may be gathered from some random instances. What wonderful ancestors would be constructed for the crinoids by a zoologist who only knew the life-history of *Antedon!*

The lowest portion of the family tree would have to present armless crowns, composed only of five basal and five oral plates, set on a stalk; then would follow genera with five large basals, five tiny radials, and five stout massive orals; then forms with five arms, at first short and later on simply branched; and so on. But I will not further elaborate the picture. All know that it does not in the remotest manner agree with the facts of palæontology. What zoologist would conclude from the developmental history of the recent sea-urchins, that the regular forms preceded the irregular, or again that the former had fossil ancestors of the type of the Perischoëchinidæ and Bothriocidaridæ? In the ontogeny of the cœlenterates there is no certain indication of the former existence of Cyathophyllidæ or Cystiphyllidæ. No observations of embryology would warrant our imagining the former existence of graptolites or stromatopores. No stage in the development of any living brachiopod informs us that numerous spire-bearing genera lived in Palæozoic and Mesozoic times. These few instance might easily be multiplied; but they may suffice to show how trivial are the discoveries concerning existence in earlier periods of earth-history that can follow from ontogenetic researches alone.

A further, indeed the practical, reason why ontogeny bears so slight a relation to geology and palæontology, lies in the fact that the earlier stages of development, with which modern embryology almost exclusively occupies itself, are not capable of preservation in the rocks, and that we can, therefore, never expect to find their fossil archetypes. The changes that occur between the embryonic and the completely adult stages have, at least among invertebrates, not yet received the attention they deserve, and it is these very changes that are of special interest to the palæontologist.

In spite of these hindrances, fossil embryonic types are not entirely wanting, even among invertebrates. The palæozoic Belinuridæ are bewilderingly like the larvæ of the living *Limulus;* the Pentacrinoid-larva of *Antedon* is nearer many fossil crinoids than is the full-grown animal; certain fossil sea-urchins permanently retain such features as linear ambulacra and a pentagonal peristome, which characterise the young of their living allies; among Pelecypoda, the stages of early youth of oysters and Pectinidæ may be compared with palæozoic Aviculidæ. Among brachiopods, according to Beecher, the stages which living Terebratulidæ pass through in the development of their arm-skeleton correspond with a number of fossil genera. Among completely distinct groups also, ontogenetic characters have been successfully traced. The beautiful researches of Hyatt, Würtenberger, and Branco have shown that all ammonites and ceratites pass through a goniatite-stage, and that the inner whorls of an ammonite constantly resemble, in form, ornament, and suture-line, the adult condition of some previously existing genus or other.

Series of forms whose successive links correspond with successive stages of development in their youngest, still existing representatives are the only ones that furnish us with an incontestable picture of the path along which any given assemblage has evolved. These are the kind of genealogical trees that form the worthy goal of palæontology. From them a natural system will arise of its own accord. But from this goal we are unhappily still far removed. As a rule our palæontological trees lack an ontogenetic foundation, and that the foundation itself may be constructed in an arbitrary manner is best shown by the unsatisfactory condition of our ammonite-literature.

The time, it seems to me, has not yet arrived for the thorough reform of zoological classification on a phylogenetic basis. Among Protozoa and Cœlenterata there are absolutely no satisfactory fixed points for the phyletic arrangement of the various groups. Among Echinodermata it is proved that the correspondence in embryonic development between Asterozoa and Echinozoa is evidence of a common origin; but the classification of the various Classes is as yet affected only to the smallest extent by the facts of ontogeny and phylogeny. The union of Bryozoa and Brachiopoda into a special phylum—the Molluscoidea, and their connection with the worms, depend entirely on embryological comparison: in their later development the two Classes go so far apart that we can find no further parallel between them; and although the beautiful researches of Beecher, Clarke, and Schuchert on the phylogeny and ontogeny of the Brachiopoda will furnish a solid foundation for a new and better classification of the Class, the first adumbration thereof is still somewhat doubtful. On the other hand, researches on the development of the shell in Mollusca are, without doubt, full of promise. What results we have to expect in this field is shown by the labours of Jackson, Hyatt, and Branco, though it must be confessed they still afford no sufficient basis for a classification of the pelecypods and cephalopods.

Palæontology has made its deepest mark in the classification of the Vertebrata. Here we frequently come across firmly-rooted genealogical trees. Phylogenetic and ontogenetic facts have effected the removal of the order Solipedia and the natural grouping of the ungulates. The discovery of the fossil Condylarthra and Creodontia has brought to light unlooked-for relationships between ungulates and carnivores. The remarkable fauna of the Puerco Beds contains, according to Cope, almost completely indifferent mixed types, which cannot be considered either as true ungulates, beasts of prey, or rodents, nor can they even be regarded as typical Condylarthra, Creodontia, or Lemuria: scarcely can they be dovetailed into the framework even of a geological classification, since they show relationships in the most varied directions.

And here we approach an important question of principle. The

larger categories in botany and zoology are almost exclusively based on the investigation of forms that still exist; and it is only in those divisions where the fossil forms surpass the recent ones, in number or in variety of organisation, that they, too, have been taken at all into account. As a rule it has been thought good enough to wedge in the extinct Orders or Families between the groups erected by the botanists and zoologists. Thus the foundations of the system remain intact. It is only recently that attempts have been made to reconstruct individual divisions of the zoological system, to a certain extent from below, on a palæontological basis. Thus, Scudder has established a Sub-Class, Palæodictyoptera, for all palæozoic insects, because they possess a series of common, indifferent characters, and show as much morphological correspondence one with another as they do with the later Orthoptera, Neuroptera, and Hemiptera, whose predecessors can already be clearly recognised among the palæozoic forms, although they have not yet attained the complete differentiation of their later descendants. Could we resurrect the numerous genera of the Puerco Beds and place them among our fauna of to-day, we should doubtless arrange them in one common Order, more or less corresponding to the marsupials; for, like the marsupials, they possess characters that, at all events, point in the direction of Orders more clearly differentiated later on, in which Orders we are at present accustomed to enrol them.

If the zoological and botanical systems were now to be created for the first time, they would in many respects probably assume a different appearance. They would have to represent clearly the natural relationship and the derivation of the organisms. The geologically oldest representatives of any of the larger assemblages, which are as a rule also the most generalised and most primitive, would have to be united under a special name, and would be regarded as the common root of the Orders, Families, Genera, etc., proceeding from them. But it is only in a few cases that palæontology could furnish the materials required for a reform of this kind. As a rule, and especially among the invertebrates, the primitive generalised types are missing, and we should be obliged to begin with those branches and twigs from our stems which are already more clearly differentiated, and of which the majority stretch down as far as the creation of our own day. Here again, then, we should be led to ground our classifications on those organisms of which we were in a position to investigate not merely certain fossilisable elements, but the whole anatomy, physiology, and embryology.

The function of classification, however, is not only to arrange organised beings according to their relationship, but also to facilitate our survey of life's infinite variety of form. It was to this intent that the older systematists constructed their various categories. They it is that have historic rights; and just as little as we geologists are inclined, without urgent need, to alter the historic conceptions and

the divisions that have been handed down to us, just so little is it advisable to be incessantly remodelling the systems of botany and zoology. The doctrine of descent has, of course, violently shaken the solid framework of the older classification. The ideas—variety, mutation, species, genus, family, order, etc.—have become indefinite and unstable; the boundaries between the systematic groups are constantly being displaced, and bonds are burst that were once tightly bound. An important part is played to-day by subjective opinions, and when I think of the anxiety with which we elders—we who received our scientific education before the Darwinian era—proceeded to found a new species or genus, and compare it with the light-hearted manner in which to-day species, genera, families, and orders are set up and again put down, I am herein most forcibly impressed by the difference between then and now. The domination of the Linnæan and Cuvierian principles threatened systematic biology with soulless paralysis: the unbridled subjectivity of recent times may easily lead to anarchy. When, after investigating a certain number of forms, every author feels called upon to reform the classification, and where possible to introduce a new terminology, then arises the danger that we shall lose our comprehensive survey of the richly varied organic world, and that we shall use a language intelligible only to the most narrow specialists and repellent to every layman.

With this warning let me conclude. The Theory of Descent has penetrated the descriptive branches of natural science with new ideas, and set before them a nobler goal; but we should never forget that it remains only a theory, and one that has to be proved. I have tried to make plain how greatly it is indebted for its establishment to palæontological research; only I dare not conceal how many gaps are constantly brought to light in the very process of our argument. Science strives in the first place for truth. And the more clearly we keep ourselves conscious of the insecurity of the foundation on which our scientific theories rest, the more actively shall we bestir ourselves to strengthen it by new observations and new facts.

KARL V. ZITTEL.

II.

Field-Club Work in Ireland.

ONE of the most cheerful features accompanying the spread of educational influences throughout Ireland is the support given to societies whose aim is the study of things pertaining to the island. The ancient and still living language, the pre-Christian tombs and dwellings, the early churches, the Norman fastnesses, and, above all, that art of ornamentation which is recognised throughout Europe as essentially Irish, all exercise their local fascination, and all thus tend to bring about that collaboration of intelligence which forms the only true foundation of national life. Such researches, moreover, cannot fail to encourage the study of history on a scientific basis, a fact which may in time atone for the omission of the subject from the curriculum of the National schools. As an example of how scientific enquiry, with its painstaking observational methods, may go to the root of the conditions of life in any district, I may cite the remarkable paper on "The Ethnography of the Aran Islands,"[1] by Professor Haddon and Dr. C. R. Browne. This is, we may hope, only the first of a series of dispassionate enquiries into the racial and tribal characteristics of the people of Ireland; and it will serve as a model for similar studies in other countries, however "civilised" the community may be. Dr. Browne has already followed the matter up by a study of "The Ethnography of Inishbofin and Inishshark, County Galway"[2]; and the two papers, covering 116 octavo pages, reveal to us what a charming field lies open in "the proper study of mankind." The observation of man, amid his varied surroundings, especially where he has long been subject to the same controlling influences, is, as far as our islands go, a much neglected branch of natural history, and we know more of many Pacific aborigines than of ourselves. The Dublin Anthropometric Committee, in connection with Trinity College and the Royal Irish Academy, has already made a brilliant start towards remedying this defect.

But probably a quietly spreading movement, mainly guided by those who modestly call themselves "amateurs," is doing more towards a complete knowledge of Ireland than even any of the older and more professional societies could effect. I refer to the growth in

[1] *Proc. Royal Irish Acad.*, ser. 3, vol. ii. (1893), p. 786.
[2] *Ibid.*, vol. iii. (1894), p. 317.

Ireland of Naturalists' Field Clubs, which foster the observation of local details and of those seemingly trivial points of difference, the sum total of which forms the natural history of the island. The specialist must be called in to correlate the observations and to decide upon the true nature of the objects collected by the members, but the existence of the field-clubs vastly widens the area over which his material can be procured.

Mr. R. Lloyd Praeger[3] has recently retraced the history of the four associations that are already in existence. The Belfast Naturalists' Field Club, which now numbers some 500 members, admits archæology as well as natural history within its scope; but in its published work it is clear that the more popular study has never been allowed to overshadow the purely scientific branches. The Belfast Club arose in 1863 among the students who had been attracted by Professor R. Tate's courses of lectures in connection with the Science and Art Department. The appendices to its annual reports have now been published as a separate volume, and form a work of constant reference for Irish geologists, botanists, and zoologists. Mr. Stewart, one of the original members, is still ready, in his official position at the Natural History Society's Museum, to give help to the large body of younger workers, field botany having always been a favourite subject in the north. Messrs. C. Lapworth and Wm. Swanston published their famous researches on the graptolite beds of Co. Down in the *Reports* of the Belfast Field Club; and Mr. Joseph Wright contributed, and still contributes, to the same publication his observations on fossil and recent Foraminifera. The presence of serious workers like these in the industrial centre of the north has given a distinct stimulus towards scientific investigation; and the foundations of their published work have been laid, in the manner of true naturalists, during long excursions by field or flood, in the open air. The indoor meetings of the club during the winter provide for the thorough discussion of results; and the excursions during the fairer season at least serve to introduce the members to the beauties of the country near their homes. The artistic work of that keen observer and photographer, Mr. Robert Welch, has already set permanently on record hundreds of antiquarian and scientific objects which have been thus examined. Latterly, one excursion, lasting several days, has been made to some point not accessible within the limits of a single day. The members participating in this return with abundant material collected from shore and heath and mountain, and with photographs illustrating the details of the geology of the district studied. The days chosen for this excursion include those set apart in the north of Ireland for reviving the political animosities of the Boyne. And thus the Naturalists' Field Club is sustaining the best traditions of science in ignoring completely all traditional differences of class or creed or race.

[3] "The Irish Field Clubs," *Irish Naturalist*, vol. iii. (1894), pp. 141, 211, and 247.

For many years Belfast had to remain content with having set forth an example; but in 1885 the exertions of Professor A. C. Haddon in Dublin resulted in the development of a second field-club. From this, considering the other societies existing in the capital, archæological subjects are wisely excluded; and the tenth year of the club's existence sees it in a sound and flourishing condition. While in Belfast the professional element has of late remained almost too much in the background, in Dublin the danger has lain in the opposite direction, and the cordial coöperation of workers already known in the Royal Dublin Society and the Royal Irish Academy no doubt tended at one time to restrain the ardour of the amateur. But at present the Dublin Naturalists' Field Club has a vigorous and independent life, and has been fortunate in securing as its secretary Mr. R. Lloyd Praeger, who acted for some years in the same capacity for the older society in Belfast. The general plan of work in the two clubs is similar, the winter meetings of the Dublin one being held in the handsome and classic rooms of the Royal Irish Academy.

In 1892, Mr. J. L. Copeman suggested the formation of a similar club in Cork; and Mr. F. Neale also succeeded, later in the same year, in founding a fourth field-club in Limerick.

The publication of that bright and well-printed journal, the *Irish Naturalist*, which is now in its fourth annual volume, has provided a means of communication between all the clubs, and between them and many older natural history societies. The Belfast Club still issues independent *Proceedings* of its own, but its meetings are chronicled in the pages of the *Irish Naturalist*. The February number of this journal for the present year bears striking evidence of the work that is quietly going on, even during the pressure of ordinary field-excursions. Mr. G. H. Carpenter, President of the Dublin Field Club, there describes the "Animals found in the Mitchelstown Caves,"[4] on the occasion of a visit lasting two hours, during a joint excursion of the three southern field-clubs. In the same number, Mr. J. N. Halbert discusses the insects collected above ground during the three days of this joint excursion to Fermoy and Mitchelstown; and these two papers show how scientific knowledge may be distinctly promoted on such occasions, despite those serious persons who are apt to look on a field-day as a sort of sanctified picnic.

After all, and especially in a country where scientific training is not easily obtained, the educational side of field-club work is at the outset of paramount importance. The training of the eye in the open country is the training of the whole individual, body and soul. And the feeling that in every county there are observers with the same aims, the same enjoyments, ready to assist, to correlate, to confer, leaves no beginner to cool his enthusiasm in discouragement. Already a Field-Club Union has been formed, through the agency of

[4] *Irish Naturalist*, vol. iv. (1895), p. 25. A full account of this paper was given in NATURAL SCIENCE for March, vol. vi., p. 148.

which the prominent workers of one club may be sent on missions to the members of others; and the Conference of the four existing clubs, which is to take place at Galway, and to occupy eight days in the present summer, will probably result in the foundation of still further associations. The Belfast Naturalists' Field Club, always in the van, has recently organised courses of lectures and practical work for its members, to provide them with a sound basis for their observations in the field; and this scheme promises to be a most healthy factor in the educational system of the north. Already the naturalist in Ireland would feel lost without the warm and ready fellowship of the field-clubs; while every year these organisations are adding solidly to the natural history of the least known and perhaps the most fascinating of our British Isles.

GRENVILLE A. J. COLE.

III.

The Last of the "Challenger."

THERE is a little picture on p. 1,608 of this last volume[1] of the "Challenger" Reports, full of pathetic meaning to all who love the sea, to every naturalist who has gone down to the sea in ships. It represents H.M.S. "Challenger" in 1895, a dismantled hulk, the

> "last scene of all,
> Sans teeth, sans eyes, sans taste, sans everything.'

It is hard to realise that we have reached the last of the noble volumes that chronicle the doings of the "Challenger." Twenty-three years ago she sailed on the great voyage that has made discovery of regions of our planet comparable only with those of Columbus, Gama, and Magellan. Little more than a quarter of a century ago three-fifths of the globe, covered by the ocean, was barely known to us, and the "Challenger" and other expeditions have now broken up this great deep of ignorance, with results "measureless to man," but set forth in a series of volumes, the mere bulk of which conveys an adequate impression of their gravity. There are two classes of men who ought to inspect these volumes: the grumbler, belonging to the *demi-monde* of science, who wails that the British "Government does nothing for science," who points to this foreign undertaking and that foreign state-aided system of education in science; and the "pure and blameless taxpayer" who wants to see his money's worth, something not bricks and mortar, that makes for national progress in discovery. The intelligent foreigner could probably help us all to a better appreciation of the magnitude of our great scientific institutions, not the least of which has been the "Challenger" voyage and the recording of its oceanographical results.

The first portion of this volume is devoted by Dr. John Murray to a history of oceanography, dealing successively with an account of the aims and limits of oceanography, a historical treatment of the

[1] "Report on the Scientific Results of the Voyage of H.M.S. 'Challenger.'"
"A Summary of the Scientific Results," by John Murray.
"Report on the Specimen of the Genus Spirula collected by H.M.S. Challenger," by the Right Hon. T. H. Huxley, F.R.S., and Professor Paul Pelseneer.
"Report on Oceanic Circulation," by Alexander Buchan, M.A., LL.D., F.R.S.E. 2 vols. Printed for H.M. Stationery Office, 1895.

views of the ancients, beginning with the Phœnicians whose mercantile expeditions were of such astonishing range, and the Greeks who first directed attention to the scientific problems of the ocean, and of the views entertained during the Dark Ages, the Middle Ages, and the Renaissance; next, with the progress of knowledge from Magellan to Cook—from Cook to the "Challenger" and the subsequent expeditions. This account, filling over a hundred of the familiar pages of the "Challenger" Reports, is most clearly and attractively written. It is illustrated with a series of coloured maps showing the progress of our knowledge, the world-maps of the ancients being of particular interest. I should greatly like to see this account made accessible to many readers as a separate publication. From its simple and fascinating style it would even make a school-book of the kind that compels study. A school-boy, to my knowledge, broke off a bloodthirsty tale for the attractions of this chapter accidentally discovered, and he read it all. It is a triumph of the literary exposition of a subject that might have been a dreary piece of pedantry. It is to be hoped that Dr. Murray may be prevailed upon to adapt this wonderful narrative to a wider circle than the readers of "Challenger" Reports. If there were added to it a more extended account of the modern voyages of research, it would make a book of the sea of the highest educational effect on a maritime people.

These great voyages of discovery did nothing for the deep-sea until the middle of this century, and both biologists and geologists have to thank telegraphic enterprise for the first impulse towards a systematic exploration of the floor of the ocean. The desire to effect telegraphic communication between Europe and America led to the birth of the modern science of oceanography, which until then reckoned only with the surface and imperfectly with circulation. The apparatus of deep-sea work had to be invented, and with steady progress in this invention very exact observations can be made, though in all cases such observations are, from the nature of the case, indirect. No better example is likely to be found of the influence of progress in one science on progress in another than this exploration of the ocean resulting from discoveries in telegraphy. Let it not be forgotten, however, that telegraphy was only the opportunity. The first self-registering thermometer was made about 1757 by Cavendish, who suggested that it might be applied to ascertaining "the temper of the sea at great depths." It was so applied by Irvine in 1773. De Saussure and others continued such observations. Again, specimens from the great depths were obtained by Sir John Ross in his voyage to Baffin's Bay 1817–18, and by his famous nephew, Sir James Ross and Dr. (now Sir Joseph) Hooker on the Antarctic voyage, who carried out remarkable deep-sea soundings.

There follows next a general summary of the scientific observations and results at each of the "Challenger" observing-stations, making, in fact, a scientific log of the voyage. This log, as we may

call it, occupies, with its details, the greater portion of both parts of this volume, and it is copiously illustrated with charts of the route beautifully engraved. At the first station the first attempts were made at sounding and dredging, the object being chiefly to exercise the seamen in the use of the apparatus. We read: "At 8.10 a.m. took first sounding, and made first attempt at dredging. In hauling in, the sounding line carried away, and a thermometer was lost. At 1 p.m. the dredge came up capsized," etc. Such was the beginning! However, a subsequent attempt at this station rewarded the voyagers with two new species, one being a new generic type; and these were never obtained elsewhere. It would be impossible to exaggerate the value of this great record of the voyage, enriched with notes from the author's own journal and the manuscript journals of Moseley and Willemoes-Suhm, and, of course, based on the official log and the published reports. It forms the companion volume to the Narrative.

At the end of this great summary it is analysed in a series of tables illustrating the bathymetrical and geographical distribution of the organisms captured. The interest culminates in Dr. Murray's general observations on the distribution of marine organisms, where his extraordinary grasp of the bewildering mass of facts and observations is focussed and his views lucidly expounded. It would be vain to attempt here even an outline of this colossal labour, but I cannot resist making an extract dealing with Dr. Murray's speculative views on the determining influence of the origin of climate on distribution. Commenting on the strong resemblance between the north and south polar marine faunas, and the absence of similar forms in a high proportion of cases from the intervening tropical belt—a resemblance extending to the pelagic flora, and even, as I have shown elsewhere,[2] to the more variable coast marine flora—the author submits the following daring hypothesis. "In early mesozoic times cooling at the poles and differentiation into zones of climate appear to have commenced, and temperature conditions did not afterwards admit of coral reefs in the polar area. But the colder and hence denser water that in consequence descended to the greater depths of the ocean carried with it a large supply of oxygen, and life in the deep seas became possible for the first time. There have been many speculations as to how a nearly uniform temperature could have been brought about in sea-water over the whole surface of the earth in early geological ages, as well as to how sufficient light could have been present at the poles to permit of the luxuriant vegetation that once flourished in these regions. The explanation that appears to me the most satisfactory is the one which attributes these conditions to the very much greater size of the sun in the early stages of the earth's history—an idea first introduced into geological speculations by Blandet, who likewise discussed the relations of Arctic and Antarctic faunas—together with the greater amount of aqueous

[2] *Phycological Memoirs*, part iii., 1895.

vapour in the atmosphere and the greater mass of the atmosphere." He proceeds: "The pelagic algæ, radiolaria, and foraminifera above referred to are probably the but slightly modified descendants of a very ancient, universal, pelagic fauna and flora. Life, in its simplest form, most likely first appeared in pre-Cambrian times in the detrital matters laid down about the mud-line, when the mud-line generally was not so deep, and the land surfaces were more extensive than at the present time. From these simple forms, which would occasionally be carried into the superincumbent waters, the pelagic protophyta and protozoa, which peopled the surface-waters of the pre-Cambrian ocean, were most probably derived." It is impossible here, as I have said, to give even the merest glance at the facts and arguments cited in support of these hypotheses. I believe I am right in saying that Dr. Murray arrived independently at the view ascribed by him to Blandet, and that his discovery of Blandet's priority was made after the idea had shaped itself in his mind, though he puts forward no claim to it here. It is, however, one that will meet with criticism and discussion—a discussion that cannot fail to help on the process of digesting farther the great granary of facts preserved in this volume.

There follows a gigantic index of the genera, sub-genera, species, and varieties, which serves also as an index to the species procured by the "Challenger," described and referred to in the Reports, since on looking up the pages in the station lists the reader will find references to the parts in which the species are fully described. There is also a general index to the summary.

As appendices there are printed the Report on *Spirula*, by Mr. Huxley and Professor Pelseneer, with Mr. Huxley's beautiful drawings, and the report by Mr. Buchan on "Oceanic Circulation," illustrated with sixteen double-page maps. Perhaps Mr. H. N. Dickson, or some other competent authority, will give us a brief account of this splendid monument of Mr. Buchan's labour and research. No student of the present distribution of organisms can dispense with this great memoir, but all should learn in brief its main teaching.

It is difficult to refrain from extending to Dr. John Murray personal congratulations on the completion of this great labour of editing and of writing the "Challenger" Reports. From his brief editorial notes prefixed to the present volume the reader will learn the kind of obstacles he has had to surmount. All naturalists now know of the perseverance and brilliant success of his great achievement. This has been rewarded mostly by the recognition of foreign academies. The fountains of honour in this country will, it is to be noped for their own credit, now begin to flow towards the editor (after the lamented death of Sir Wyville Thomson) of the "Challenger" Reports, the author of some of the finest work embodied in them, one of the most indefatigable of the naturalists who sailed on the memorable voyage.

GEORGE MURRAY.

IV.

Some Definitions of Instinct.

THE phenomena of instinct are of interest both to biologists and to psychologists; who respectively approach them, however, from different standpoints. Whether the divergences of opinion concerning these phenomena, and the diversities of definition of the terms "instinct" and "instinctive," are mainly due to this cause, it is perhaps difficult to decide. That marked differences do exist is only too obvious.

1. **Relation of Instinct to Consciousness.**—"Instinct," says Professor Claus,[1] "may be rightly defined as a mechanism which works unconsciously, and is inherited with the organisation, and which, when set in motion by external or internal stimuli, leads to the performance of appropriate actions, which apparently are directed by conscious purpose." Here, then, we have instinct defined as essentially unconscious. Mr. Herbert Spencer[2] regards instinct in its higher forms as probably accompanied by a rudimentary consciousness; but he does not consider the presence of consciousness essential. Professor Baldwin speaks[3] of a "low form of consciousness which has not character enough to be impulsive"; while Professor Calderwood[4] holds that instinctive activities cannot be attributed to mental power. "The entire chapter on Instinct in Darwin's 'Origin of Species' must," he says, "be read in an altered form, consequent on the deletion of the references to 'mental faculties.'"

On the other hand, Romanes commences his definition of instinct with the words[5]: "Instinct is reflex action into which there is imported the element of consciousness." "The term comprises," he says, "all those faculties of mind which are concerned with conscious and adaptive action, antecedent to individual experience." "The stimulus," he adds, "which evokes an instinctive action is a perception." Professor Wundt also emphasises the conscious accompaniments of instinctive activities, which, he says,[6] "differ from

[1] "Text-book of Zoology." Eng. Trans., vol. i., p. 94.

[2] "Principles of Psychology," ch. xii.

[3] "Text-book of Psychology." *Feelings and Will*, p. 308. He also speaks of instincts as "inherited motor intuitions," p. 311.

[4] "Evolution, and Man's Place in Nature," p. 190.

[5] "Mental Evolution in Animals," p. 159.

[6] "Lectures on Human and Animal Psychology," Eng. Trans., p. 401.

the reflexes proper in this, that they are accompanied by emotions in the mind, and that their performance is regulated by these emotions."

Thus, even if we exclude the extreme views of those who hold that instinctive activity is due to connate ideas, and inherited knowledge,[7] there is a wide range of opinion on this head.

2. **Relation of Instinct to Impulse.**—Professor Wm. James speaks[8] of "instinctive or impulsive performances." "Every instinct," he says, "is an impulse," and he implies that every impulse is instinctive. Professor Wundt[9] and Herr Schneider[10] also regard instinctive activities as prompted by impulse; the last-named author distinguishing between sensation-impulses, perception-impulses, and idea-impulses. But other writers use the term in a more restricted sense. Professor Höffding, though he holds[11] that "instinct is distinguished from mere reflex-movement by the fact that it includes an obscure impulse of feeling," also tells us[12] that "impulse [here used in the narrower sense] involves a contrast between the actual and a possible or future. This," he adds, "is what distinguishes it from reflex-movement and instinct, where the excitation may perhaps cause a sensation, but where no idea asserts itself of what must follow." Professor Baldwin distinguishes[13] between those stimuli and the reactive consciousness which, as originating mainly from within, may be called in general *impulsive*, and those which, as originating mainly from without, may be termed *instinctive;* but he admits that the distinction is inexact.

In introducing therefore into a description of instinctive activities any reference to impulse, the exact sense in which this word is employed itself needs definition.

3. **Relation of Instinct to Intelligence and Volition.**—Mr. H. Spencer describes[14] instinct as compound reflex-action. Although he states clearly[15] that "the actions we call rational are, by long-continued repetition, rendered automatic and instinctive"; yet his main thesis is[16] that instincts are developed on the path of upward development from reflex-action toward volitional activity. Others, who are not prepared to follow Mr. Spencer in his main contention, still regard instinctive actions as essentially involuntary. Such views may be contrasted with the opinions of G. H. Lewes[17] and Herr Schneider,[18]

[7] "Instinct and Acquisition." *Nature*, vol. xii., p. 507. Oct. 7, 1875. The passage is quoted *infra*, p. 325.

[8] "Principles of Psychology," vol. ii., p. 382. See also the passage quoted *infra*, p. 325.

[9] *Op. cit.* [10] "Der Thierische Wille."

[11] "Outlines of Psychology," p. 91.

[12] *Op. cit.*, p. 322. *Cf.* also H. R. Marshall's "Pain, Pleasure, and Æsthetics," pp. 275–277.

[13] *Feelings and Will*, p. 304.

[14] "Principles of Psychology," ch. xii., § 194.

[15] *Op. cit.*, § 204. [16] *Op. cit.*, § 211.

[17] "Problems of Life and Mind." *Instinct.*

[18] "Der Thierische Wille."

who regard instinct as due to lapsed intelligence; habits formed under intelligent guidance being inherited in the form of instincts. Professor Wundt seems to go yet further when he says[19]: "Instinctive action is impulsive, that is voluntary action; and, however far back we may go, we shall never find anything to derive it from except similar, if simpler, acts of will. The development of any sort of animal instinct, that is to say, is altogether impossible unless there exists from the first that inter-action of external stimulus with affective and voluntary response which constitutes the real nature of instinct at all stages of organic evolution." Thus, while Mr. Herbert Spencer regards instinct as primarily not yet voluntary; and while many writers regard it as no longer voluntary; Professor Wundt asserts that it is at no time involuntary.

4. **Relation of Instinct to Habit.**—The word "habit," like so many others in this connection, is used in different senses. Many writers describe all the activities of animals as their habits. In this sense we speak of habit as correlated with structure. But the term is generally used in psychology in a more restricted sense, and is applied to those activities which have become stereotyped under the guidance of individual control. A habit is, in this acceptation of the term, an acquired activity, the constancy of which is due to frequent repetition by the individual, in adaptation to special circumstances; and a distinction is drawn between such habits, as individually acquired, and instincts as connate.[20] Those who accept the Lamarckian hypothesis of the origin of instincts through "lapsed intelligence" regard them as the connate effects of the inheritance of acquired habit. Darwin[21] and Romanes[22] believed that instincts were in part due to this mode of origin. Professor Wundt, however, gives to the term a wider meaning, and so defines instinct as to include acquired habit. "Movements," he says,[23] "which originally followed upon simple or compound voluntary acts, but which have become wholly or partly mechanised in the course of individual life, or of generic evolution, we term *instinctive* actions." In accordance with this definition, instincts fall into two groups. Those, "which, so far as we can tell, have been developed during the life of the individual, and in the absence of definite individual influences might have remained wholly undeveloped, may be called *acquired* instincts."[24] They have become instinctive through repetition. "To be distinguished from these acquired human instincts are others, which are *connate*."[25] "The laws of practice suffice for the explanation of the acquired instincts. The occurrence of connate instincts renders a

[19] "Lectures on Human and Animal Psychology," p. 409.

[20] *See*, for example, Professor Sully, "The Human Mind," vol. ii., p. 184.

[21] "Origin of Species," p. 206; "Descent of Man," vol. i., p. 102, quoted in "Mental Evolution in Animals," p. 264.

[22] "Mental Evolution in Animals," p. 200.

[23] "Lectures on Human and Animal Psychology," p. 388.

[24] *Op. cit.*, p. 397. [25] *Op. cit.*, p. 399.

subsidiary hypothesis necessary. We must suppose that the physical changes which the nervous elements undergo can be transmitted from father to son . . . The assumption of the inheritance of acquired dispositions or tendencies is inevitable if there is to be any continuity of evolution at all. We may be in doubt as to the extent of this inheritance: we cannot question the fact itself."[26] "Darwin's explanation of the development of instinct as being mainly the result of passive adaptation seems," says Professor Wundt, "to contradict the facts."[27] Now the majority of writers on instinct distinguish it, as we have seen, from individually-acquired habit. And it is hardly necessary to state that Professor Wundt's explanation of the origin of connate instincts on Lamarckian principles, is not accepted by Professor Weismann and his school. "I believe," says Professor Weismann,[28] "that this is an entirely erroneous view, and I hold that all instinct is entirely due to the operation of Natural Selection, and has its foundation, not upon inherited experiences, but upon variation of the germ." In view of the biological controversy as to the inheritance of acquired characters, it would seem advisable so to define instinct as not in any way to prejudge the question of origin.

5. **The Instincts of Man.**—"The fewness and the comparative simplicity of the instincts of the higher animals," said Darwin,[29] "are remarkable in contrast with those of the lower animals." Romanes[30] held that "instinct plays a larger part in the psychology of many animals than it does in the psychology of man." "Recent research," says Professor Sully,[31] "goes to show that though instinctive movement plays a smaller part in the life of the child than in that of the young animal, it is larger than has been generally supposed." Professor Preyer[32] tells us that "the instinctive movements of human beings are not numerous, and are difficult to recognise (with the exception of the sexual ones) when once the earliest youth is past."

On the other hand, Professor Wundt[33] regards human life as "permeated through and through with instinctive action, determined in part, however, by intelligence and volition." And Professor James tells us[34] that "man possesses all the impulses that they (the lower creatures) have, and a great many more besides." The higher animals have a number of impulses, such as greediness and suspicion, curiosity and timidity, all of them "congenital, blind at first, and productive of motor reactions of a rigorously determinate sort. *Each of them, then, is an instinct*, as instincts are commonly defined. *But they contradict each other*—'experience' in each particular opportunity of application usually deciding the issue. *The animal that exhibits them*

[26] *Op. cit.*, p. 405.
[27] *Op. cit.*, p. 409.
[28] "Essays" (1889), p. 91.
[29] "Descent of Man," vol. i., p. 101.
[30] "Mental Evolution in Man," p. 8.
[31] "The Human Mind," vol. ii., p. 186.
[32] "The Mind of the Child": The Senses and the Will, p. 235.
[33] "Lectures on Human and Animal Psychology," p. 397.
[34] "Principles of Psychology," vol. ii., pp. 392, 3. Italics the author's.

loses the 'instinctive' demeanour, and appears to lead a life of hesitation and choice, an intellectual life; *not, however, because he has no instincts—rather because he has so many that they block each other's path.*" This is in tolerably marked contrast with the statement of Darwin's which stands at the head of this section!

6. **The Plasticity and Variability of Instinct.**—"Though the instincts of animals," said Douglas Spalding,[35] "appear and disappear in such seasonable correspondence with their own wants and the wants of their offspring as to be a standing subject of wonder, they have by no means the fixed and unalterable character by which some would distinguish them from the higher faculties of the human race. They vary in the individuals as does their physical structure. Animals can learn what they did not know by instinct, and forget the instinctive knowledge which they never learned, while their instincts will often accommodate themselves to considerable changes in the order of external events." It will be noticed that there are here two groups of facts: (1) variations, analogous to variations in physical structure; and (2) accommodations to changes in the external order of events. Professor James[36] says, "the mystical view of an instinct would make it invariable"; and he formulates two principles of non-uniformity of instincts, (1) that of the inhibition of instinct by habits; and (2) that of the transitoriness of instincts. The variation analogous to that of physical structure is not here explicitly recognised. Romanes, who defines[37] instinct as a generic term comprising "all those faculties of mind which are concerned with conscious and adaptive action, antecedent to individual experience . . . and similarly performed under similar and frequently recurring circumstances by all the individuals of the same species," appears to lay stress on their invariability; but his subsequent treatment[38] shows that he fully recognised the connate variability of instinct. Under the head of "plasticity" he also[39] insisted on "the modifiability of instinct under the influence of intelligence." He quotes, with approval, Huber's exclamation: "How ductile is the instinct of bees, and how readily it adapts itself to the place, the circumstances, and the needs of the community." There seems, however, some want of logical consistency in first defining instinct as connate and antecedent to individual experience, and then implying that, as modified under the influence of experience, it still remains instinct. For example, Romanes says[40]: "There is evidence to show that the knowledge which animals display of poisonous herbs is of the nature of a mixed instinct, due to intelligent observation, imitation, natural selection and

[35] *E.g.*, Douglas Spalding. *Instinct and Acquisition. Nature*, vol. xii., p. 507.
[36] "Principles of Psychology," vol. ii., pp. 391–4.
[37] "Mental Evolution in Animals," p. 159.
[38] *Op. cit.*, p. 190. *Cf.* Darwin, in the same work, pp. 372 and 383.
[39] *Op. cit.*, p. 203.
[40] *Op. cit.*, p. 227.

transmission." Other writers render the term "instinct" indefinite by including the effects of individual experience. Mr. A. R. Wallace, for example, says[41]: "Much of the mystery of instinct arises from the persistent refusal to recognise the agency of imitation, memory, observation, and reason as often forming part of it. Yet there is ample evidence that such agency must be taken into account." But would it not be well, one may ask, so to define instinct as to distinguish it from these agencies; and to say that the habits or activities of animals are of mixed origin, the term instinct being reserved for particular types of connate activity?

7. **The Periodicity and Serial Nature of Instinct.**—Little need be said on this head, since most writers recognise the facts as, at any rate in many cases, characteristic of instinct. The sexual instincts, nidification, incubation, and migration, exemplify the periodic nature of instinct; and the fact that this periodicity involves internal as well as external determination suggests the rejection of Professor Baldwin's distinction between impulsive and instinctive; not because it is logically incorrect, but because there is so much overlap, many instincts involving an impulsive factor. That instincts are very often serial in their nature and involve a chain of activities is also commonly admitted, and is well brought out by Herr Schneider.[42]

8. **Suggested Scheme of Terminology.**—From what has gone before, it will be seen that there is a good deal of diversity of opinion and of definition in the matter of instinct. Let us summarise some of these diversities.

Instinctive activities are unconscious (Claus), non-mental (Calderwood), incipiently conscious (Spencer), distinguished by the presence of consciousness (Romanes), accompanied by emotions in the mind (Wundt), involve connate ideas and inherited knowledge (Spalding); synonymous with impulsive activities (James), to be distinguished from those involving impulse proper (Höffding, Marshall); not yet voluntary (Spencer), no longer voluntary (Lewes), never involuntary (Wundt); due to natural selection only (Weismann), to lapsed intelligence (Lewes, Schneider, Wundt), to both (Darwin, Romanes); to be distinguished from individually-acquired habits (Darwin, Romanes, Sully, and others), inclusive thereof (Wundt); at a minimum in man (Darwin, Romanes), at a maximum in man (James); essentially congenital (Romanes), inclusive of individually-acquired modifications through intelligence (Darwin, Romanes, Wallace).

It is scarcely probable that in the face of such divergence of opinion unanimity is yet within the bounds of reasonable expectation, and the following scheme must be regarded as provisional and suggestive. Certain points must be borne in mind in endeavouring to frame satisfactory and acceptable definitions of the terms

[41] "Darwinism," p. 442.
[42] "Der Thierische Wille" *e.g.*, p. 208.

"instinctive" and "instinct." Since the phenomena are in part biological and in part psychological, any definition should be such as to be of biological value and yet such as to be acceptable to psychologists. Since the question of origin is still *sub judice*, the definition should be purely descriptive, so as not to prejudge this question. And since the phenomena of instinct can only be rightly understood in their relation to automatism, connate and acquired, to impulse, to imitation, and to intelligence, our definition of instinctive activities should find a place in a scheme of terminology. Such a scheme is here set forth.

Connate automatism: the congenital physiological basis of those movements or activities which are antecedent to individual experience.

Physiological rhythms: congenital (or connate) rhythmic movements essential to the continuance of organic life.

Reflex movements: congenital, adaptive, and co-ordinated responses of limbs or parts of the body: evoked by stimuli.

Random movements: congenital, more or less definite, but not specially adaptive movements of limbs or parts of the body: either centrally initiated or evoked by stimuli.

Instinctive activities: congenital, adaptive, and coördinated activities of the organism as a whole; specific in character, but subject to variation analogous to that found in organic structures; similarly performed by all the members of the same more or less restricted group, in adaptation to special circumstances frequently recurring or essential to the continuance of the race; often periodic in development and serial in character.

Mimetic movements and activities: due to individual imitation of similar movements or activities performed by others.

Impulse (Trieb): the affective or emotional condition, connate or acquired, under the influence of which a conscious organism is prompted to movement or activity, without reference to a conceived end or ideal.

Instinct: the connate psychological impulse concerned in instinctive activities.

Control: the conscious inhibition or augmentation of movement or activity.

Intelligent activities: those due to individual control or guidance in the light of experience through association.

Motive: the affective or emotional condition under the influence of which a rational being is guided in the performance of deliberate acts.

Deliberate acts: those performed in distinct reference to a conceived end or ideal.

Habits: organised groups of activities, stereotyped by repetition, and characteristic of a conscious organism at any particular stage of its existence.

Acquired automatism: the individually modified physiological basis of the performance of acquired movements or activities which have been stereotyped by repetition.

There is certainly some overlap in the definitions, and it is difficult to see how such overlap is to be avoided. The physiological rhythms—such as the heart-beat, respiratory movements, and peristaltic action—are in part automatic, in the physiological sense of originating within the organ which manifests the rhythm; but they are also in part reflex. The line between reflex movements and instinctive activities cannot be a very rigid one; instinctive activities are indeed in large degree organised trains or sequences of coördinated reflex movements.

Although the psychological aspect of instinctive activities falls under the general head of impulse, yet impulse is broader than instinct—that is, if we adopt the definitions above suggested. On the one hand, some reflex movements are probably accompanied by impulse. On the other hand, when intelligent activities pass into habits through repetition, the performance of these habits is prompted by impulse. Impulse may, in fact, be either connate or acquired, and may be associated both with automatism and with control. Instinct is a form of connate impulse. As such it may be counteracted or modified by an acquired impulse due to pleasurable or painful experience. A chick, for example, which has run after and seized a cinnabar caterpillar, acquires through experience a counteracting impulse due to the disagreeable effect. The connate impulses, termed instincts, may thus be modified by acquired impulses which result from experience; but there is seldom or never a conflict of instincts, as these are above defined.

Whether the objective activities termed instinctive are *always* accompanied by the subjective connate impulse termed instinct is a question which is open to discussion.

A wider definition of instinct by which it would be synonymous with connate impulse may be suggested as an alternative to that above given. This would, perhaps, be more in accord with the popular use of the word "instinctive," but it appears to be less satisfactory as a definition of the technical term.

It is well to distinguish motives, as the determinants of deliberate acts, from the acquired impulses which are the determinants of intelligent activities as above defined. As the intelligent activity is often the outcome of a conflict of impulses, so is the deliberate act the outcome of a conflict of motives.

Mimetic activities are due to a mimetic impulse. Some of them are probably involuntary and due to connate impulse; but others are certainly due to intelligent imitation. They form a group sufficiently well-defined to warrant the distinct place assigned to them in the suggested scheme.

The habits of animals are in very many cases of complex origin. It is claimed that such a scheme of terminology as is above suggested may serve to aid us in discriminating between the several factors, instinctive, mimetic, and intelligent. The fact that many instinctive activities are subject to modification through imitation and experience, clearly indicates that they at least are accompanied by consciousness. But it is submitted that, when thus modified, they cease to be instinctive, that is, if *congenital* is to take its place as an integral part of the definition of instinct. They should be termed habits.

The distinction between congenital or connate on the one hand and acquired on the other hand is a definite one. Objectively considered, those activities, the performance of which is, so far as they are concerned, antecedent to and irrespective of individual experience and guidance, are connate, no matter at what stage of life they are performed; while those activities, or modifications of activity, which are performed as the result of individual experience, are acquired—any modification of connate organic structure correlated therewith being an acquired character. Subjectively viewed, those impulses which are nowise dependent on antecedent experience of pleasure or pain are connate; while those which are due to individual experience are acquired. In any given case of animal habit it may be difficult to determine how far it is due to connate activity, and how far there is acquired modification. But this difficulty is more likely to be overcome by observation and experiment, if the exact terms of the problem are kept clearly in view.

C. Lloyd Morgan.

V.

The New Oban Cave.

ALTHOUGH the former treasures of the English caves are admired, it is so long since anything new has been disentombed, that cave-hunting is regarded by some almost as a lost art. It is, however, probable that many of the gaps still existing in our geological history will be filled by the study of cave-deposits. The more microscopic methods of examination now coming into vogue have already revealed much that was previously overlooked. Thus, as in the case of the rich finds of the Ightham fissure, we have been able to add to our Pleistocene fauna numerous small, but none the less interesting, species. However large the cave may be, pickaxe and shovel should never be used, except for the removal of fallen rock or the breaking through of stalagmite: every particle of the uncemented material should be removed with a knife, or, at the most, by a small hammer or trowel; it should then be sifted through six or eight sieves fitting into each other, and after having been carefully examined for friable things, washed through the sieves, with a judicious application of acid when such seems necessary. The last sieve should be fine enough to stop an ostracod, the gall of a *Cynips*, or a *Chara* capsule.

Between the discoveries of Dr. Hicks in the Welsh caves and the publication of a list of the contents of the Ightham fissure, a long rest intervened. But the succeeding twelve months have been far more fruitful. Other fissures further south have been worked, and, so far as the investigations have gone, have proved quite as interesting. That part of the fissures as yet examined is not older than the Kitchen Midden period; but the closer examination of the material has revealed whole sets of tools, perhaps the most delicate ever used by man. The British Association made a grant last year for the furtherance of these researches, and the results will shortly be published.

On the top of these comes a most interesting discovery in Scotland, where the later chapters of geological history are somewhat different from our own. There, for instance, the old Palæolithic man, whose remains are entombed in almost every rod of gravel from the Wash to the English Channel, is unknown; while Kitchen Middens, which were practically unknown in the South till those just mentioned were discovered, are quite plentiful in the North. A few years ago a cave of some interest was found at the back of Mr. J. W. Higgin's whisky distillery at Oban. Recently Mr. McArthur, while blasting rock on his property in Oban Bay, broke into another cave of great promise

and interest. It is the object of the present note to give some few particulars of this cave, and for them I am mainly indebted to Mr. J. W. Higgin, the secretary of the Exploration Committee.

One of the first features to strike the eye of the casual visitor to this charming bay is the raised beach, some 35 ft. above the present one. It was upon this level that the quarrying operations were proceeding when the observant eye of Mr. McArthur fell upon a shell-bed. He immediately informed Mr. Higgin, who recognised the importance of the discovery, and, although rumour has it that many of the earlier finds were thrown away as having no connection with man, still the numbers in which they appeared soon showed that they could not possibly be mere freaks of nature. Upon exhibiting to the Archæological Society of Scotland one of the bone harpoons discovered, Dr. Anderson induced that body to supply the funds necessary for a thorough investigation. With this the severe winter interfered, but the following general facts may prove of interest.

The cave opens to the N.E., and, at its mouth, is about 20 ft. wide; it extends back about 30 ft. The roof was covered with stalactites, which, together with the overhanging rock, have all been cleared away, leaving the floor for examination. The mouth of the cave was artificially stopped by huge pieces of rock, which were placed right across it in the lower part, and subsequently occupied two-thirds of its width. The explorers have made two deep longitudinal trenches and one transverse one, and have removed some fifty cartloads of material. The excavations have revealed the following section:—

1.	Rock-debris and humus	3 ft.
2.	Shell-bed	12 to 26 in.
3.	Beach gravel	3 ft.
4.	Shell-bed	3 to 26 in.
5.	Stalagmite, sand, bones, etc.	1 ft. 8 in.
6.	Bed-rock	at 12 ft.

1. The accumulation of this extensive deposit indicates a considerable lapse of time since the deposition of the underlying shell-bed. At the close of this period, a stray visitor appears to have crawled in and made the cave his last resting-place, as his skull is there to testify.

2. This layer is extremely interesting. It seems to be the equivalent of the ordinary Kitchen Midden material, since it consists of the shells of the limpet, quantities of the large *Pecten* so plentifully found in many of the Scotch glacial deposits, oysters, mussels, cockles, sea-snails (*Natica?*), winkles, crabs, and other edible species. The limpets are said to bear evidence of having been roasted before being eaten. The various species are often found in heaps, and not irregularly mixed with other shells as on a shore. Dispersed through this bed are also the bones of fish, birds, and mammals. The latter are in the hands of Sir Wm. Furness for determination: some of the deer antlers are of very large dimensions, suggestive of

Megaceros. Bone and flint implements are also found here: the former are very numerous and include some beautifully double-barbed harpoons, one of which, 6½-in. long, with nine barbs and a rounded pierced end, was probably used as a detaching harpoon; another, not pierced, which was evidently hafted, measures 4½-in. in length and has four pairs of barbs, being very similar to a tool found in the Victoria cave. There are numerous fragments of these tools, and a quantity the use of which is not quite clear; but the repetition of the same types determines their human origin however unable we may be to decide what was their use. Some of the bone tools are of chisel form and others are suggestive of arrow tips. So preponderating are these bone tools that one might assign the deposit to a "bone age," were it not probable that the extensive use of this material is to be attributed merely to the absence of flint in that part of the country, for in the Hastings Kitchen Midden reverse conditions have produced reverse results. Flint implements have, however, been found, and this is all the more interesting from the fact that the nearest locality for this form of silica is probably Mull.

3. This bed of beach-gravel is evidently of marine origin, and was deposited by the sea when it rose to this height, or when the land and sea had a relative difference of some 35 ft. as compared with the present configuration of the country.

4. The succeeding shell-bed is another relic of human habitation; and although greatly affected and decomposed by the overlying bed and the conditions under which the latter was accumulated, it contains a large number of precious remains in the form of bone and flint implements, animal bones, and, above all, the bones of the men themselves. The human remains include one skull, two lower jaws, and several limb-bones. Exact details concerning these will be impatiently awaited; it is, however, reported that the cephalic index of the skull from the upper layer is 75, while that of the skull from the lower bed is considerably less. During this tenancy it seems that the land was sinking and the sea rising; so that against the incursions of the tide the troglodytes endeavoured to build a wall right across the mouth of the cave. But their efforts were in vain, and man was driven forth from the cave, unable to return until after the re-elevation of the land and the deposition of three feet of shingle. How long a period this represents we cannot say until a full report has been made on the contents of beds 4 and 2 respectively; and it is to be hoped that every inch of the former will be carefully examined, for it may yield many more human bones.

5. Even this lower stalagmitic layer contains treasures sealed up in it, and these may carry us back to a period of whose remoteness we have as yet no idea. This, however, we can say, that since man made the attempt to stem the incoming tide at Oban, that part of Scotland has been elevated some 35 or 40 feet.

W. J. LEWIS ABBOTT.

VI.

Bibliographical Reform and the "Zoological Record."

IN previous communications which I have made on the subject of Bibliographical Reform, principal emphasis has been laid upon such parts of the proposed system as are essentially new, to the exclusion of certain other features which are already realised, and which consequently seem almost self-evident. The task which we are undertaking is a reform in the truest sense. We seek merely to perfect what already exists; and, if we have placed before ourselves a certain organisation as the ideal towards which we are striving, it must not be supposed that we desire to cut loose from existing bibliographical undertakings, or to rival them. Just the contrary is the case. The large body of disinterested zoologists, of whom I have become the rather unwilling spokesman, seek above all to attain the closest possible harmony of action with all those who are now engaged in this arduous but necessary bibliographical work.

It is in this spirit that we now approach the *Zoological Record*, and offer it the support which an international organisation can give. But before I turn to the nature of this offer, I shall be obliged to say a few words in regard to the imperfections in the present *Record*. This is not an easy task. To one who has followed, as I have done, the history of this enterprise, who knows from numerous friendships among the Recorders the personal sacrifices that have been made for its success, who knows also the financial burden which the Zoological Society has assumed in its behalf, it is difficult to criticise frankly the product of that devotion. In point of fact, during my recent visit to England I shrank more than once from repeating what I had heard from all sides. I should not touch upon it even now, if I were not able to follow the statement with a definite proposition which it is believed will remedy those defects.

In the course of the agitation of this question, which I have carried on continuously for several years, I have naturally been able to collect a large number of opinions in regard to the need for a thorough reform. I speak, therefore, with a certain degree of confidence in stating that the *Record* is found in practice to be sadly inadequate. If anyone doubts the disinterestedness of the opinions thus gathered, I should merely point to the various notes in NATURAL SCIENCE, in *Nature*, in the *American Naturalist*, and elsewhere, written for the most part without my knowledge. They all assume the insufficiency of the existing bibliographical means. Indeed, no one has thought it

necessary to demonstrate it. I would also point to the committees nominated, some quite independently, to consider the possibilities of a reform. Looking back upon my recent hasty visit to England, it seems to me that the conviction is almost as strong in England as in other parts of the world. Indeed, were it not for the fear of violating confidences, I am sure I could base my entire plea on the statements made to me by English zoologists personally connected with the *Record*. It is needless to insist upon the entire disinterestedness of such persons, or upon their desire to raise the standard of the work. I sincerely hope it will be regarded needless in my own case as well. Let us then face the matter frankly: the *Record*, in spite of all the efforts that are being made in its behalf, is sadly imperfect, and any serious attempt to raise its standard of excellence deserves the hearty support of every English zoologist.

Before turning to consider the means of improving the *Record*, however, I desire to point out another serious defect. Here, I am sure, no one will contradict me. The maintenance of this journal imposes excessive financial burdens on the Zoological Society. Some one has said: "No publication can succeed that is not a commercial success!" I should be loth to insist upon the exactness of this maxim as universally applied, but I think that the experience of the *Record* is a singular confirmation of its justness *in petto*. What has been the answer to those who complained of the exclusion of extinct forms from most of the chapters of the *Record*? "We cannot spend more than we do. We lose £350 annually. We cannot assume a greater burden." Before this astounding statement it was obviously impossible to press the matter. No larger sacrifice can be asked. The sacrifice is already too large.

The defects of the present *Record* are of various kinds. Some are trifling matters of detail and result from a faulty system. Their elimination would require no essential change, and they may be neglected entirely for the moment. Others, such as the want of an author's index, require additional labour. It is with these more serious faults that I propose to deal, selecting from among them the most difficult of all, viz., incompleteness.

If the *Record* is to meet the needs of the zoological world, there must be the assurance that no really important paper has been omitted. For a purely morphological record, this word "important" implies, doubtless, a considerable restriction, but even then, exclusion must be very sparingly used. It is obviously undesirable that the innumerable notes, published often with no intention of contributing to knowledge, should be dragged forth from their obscurity and placed upon permanent record, where every serious student will encounter them. Such a procedure would be only an impediment, no aid. With systematic zoology, however, the case is different. Our rules of nomenclature are explicit on this point. All descriptions of new species, at least, are "important," and the *Record* fails in its object if

it overlooks them. Indeed, it is for the obscure notes that the agency of the *Record* is the most useful. The others we find without its aid.

Leaving out of account then certain purely popular articles, there remain, nevertheless, about 8,000 contributions annually, distributed over at least 800 journals, which must be recorded. Furthermore, a very large number of these papers deal with several groups of animals, and should be treated by several different recorders. With very many the title bears no indication of this diversity. Often, finally, very important incidental observations are inserted in a memoir on a totally different topic. These are commonplaces, known to everyone. If anyone care for an example, let me refer him to the *Biologische Centralblatt* for April 1, 1894, where I have given a partial bibliography, in which the most important observations are all published as incidental items.

Here, then, is the task of the present recorder. He is expected to pass in review all these titles, published in at least a dozen different languages, and to record every observation relating to his special group, whether that observation form the substance of an entire memoir, or whether it be merely an incidental note. After doing that, the recorder will be rewarded, if he is concerned with a small group, with a list of 20, 30, or even 40 titles! Personally, I doubt whether any recorder has ever been so conscientious as that, and I am very sure that having been so once, he never would be again. No! many recorders confess frankly that their own records are incomplete. Indeed, the study of the various records is a delightful exposition of comparative conscientiousness. Some recorders, I know, merely note the literature that comes to their notice, and undertake no search for titles.

What then is the remedy? Instead of each recorder having to search through the entire literature for his titles, there should be a single competent person designated to do this work adequately, and once for all. It may be replied that these are impossible conditions. What is a *competent* bibliographer? In addition to being methodical and painstaking, this person must be a trained zoologist; otherwise his allotment of the titles could never be depended upon. Moreover, he should have a reading knowledge of all the languages in which zoological papers are published. I have never met a person who could fill all these conditions. There must, then, be a staff of zoologists, not a single man. The work must be divided. However, the sum total of the work to be performed must, as I have ascertained by actual tests, be counted as requiring the entire time of one person. Obviously then, such a service organised solely by the *Record*, and with provision made for classing Bohemian, Hungarian, Polish, Russian, and such other languages as might be unfamiliar to the bibliographer, could not be realised in England for less than £300 per annum.

It is here that the value of the Central Bureau becomes

evident. Relying upon various other contributions for its principal support, the bibliographical service of the Bureau would be able to do this work for the *Record*, provided the latter could assume merely a small share of the entire expense. Indeed, an annual grant of, say £75, would, we believe, suffice.

This, however, is not yet the entire solution of the difficulty. The *Record* would doubtless be improved and the work of the Recorders made easier. We have, however, thus far only increased the expense, not lowered it. We can not even accept the solution proposed by certain Recorders to tax them all for this aid, as if they were not already underpaid! If the Recorders are relieved from part of their burden, let them rather endeavour to better their records. It is for this that their skill and knowledge as specialists are needed, not for collecting titles.

Far better is it to face the question whether a properly organised Record, which should appeal to the support of the entire zoological world, could not be made to cover in large measure its expenses. It has never been tried. The Naples Record only treats a part of the field, the Berlin Record is behind time and is merely German, the English Record is incomplete and is largely dependent on English subscribers. Now, in spite of the competition, one of these, the Berlin Record, has remained entirely self-supporting, while its English rival has been losing £350 annually.

Here, then, is the opportunity of the *Zoological Record*. The Naples Record has specialized in regard to Morphology, the London Record in regard to Systematics and Faunistics: they have by this fact almost ceased to be rivals. Let them take a slight further step. Let them join forces in order that they may together constitute a single complete Record for the whole field of zoology. Against such a combination I have the assurance that the Berlin Record would not endeavour to compete, but would gladly give up an unequal combat and aid the new undertaking to success.

This federation with the Naples Record would be an important step in introducing the London Record into the Continent; but at the present moment we can offer quite exceptional aid in this same direction. I refer to the federation of the two Records with the *Bibliographical Bulletin*, which it is proposed to publish beginning with January next. By this the entire work of recording would become a continuous process, beginning with the first rapid classification of the *Bulletin* and ending with the more perfect elaboration of the *Record*.

The new Bibliographical Bureau into which it is suggested that the *Record* might enter is being founded upon the broadest possible basis. To cite merely the case of France, where the movement chances to be the most completely organised, there is a large and influential central commission, nominated by the French Zoological Society and pledged to the support of the Bureau in its various needs. Then there are

affiliated with this central committee a score of associate members distributed among all the zoological centres of the country, and ready to bring the matter before the notice of all the local societies and to solicit their support. In general the Zoological Society of France "is resolved to aid this movement in every way possible." It is at present printing a report of its vice-president, Mr. E. L. Bouvier, on this subject, and will bring it to the notice of all its members and urge them to support the movement. The French Entomological Society may also be counted upon to aid in finding subscribers for the Bureau's publication, including, of course, the *Record*.

It must not be supposed that this proposition implies abandoning the *Zoological Record;* that would be a pity. In the first place, an injustice would be done to old subscribers to the *Record*, who have purchased it presuming that their volumes would form part of a continuous series. Secondly, it would be unjust to the Zoological Society, whose stock of back volumes would thereby become depreciated. Thirdly, it would have many inconveniences for the new journal, which would have to win these subscribers afresh, which would lose the backing and the control of the Zoological Society, and which could not, therefore, give an adequate guarantee of permanence. The founding of a new journal would, I think, be justified only as a last resource.

A better principle is to change the existing state of things as little as possible—a reform, no revolution. The editorship and the entire control of the *Record* should remain as heretofore with the Zoological Society. The English language might be conceded, and, indeed, nothing need be altered save such things as might be specified in a definite agreement to be drawn up in advance.

The principal change necessitated by the federation would be merely a slight further step in a direction already tacitly accepted. I should surely offend no sensibilities in asserting that, as a morphological review, the existing *Record* is, save a few exceptional chapters, utterly useless. For systematic zoology, on the other hand, the Naples *Jahresbericht* is of absolutely no value, and the systematic notes given with certain chapters, *e.g.*, Crustacea, are, to my mind at least, mere disfigurements. Let, then, each *Record* leave to the other that part of the field which it is the more competent to perform. This is an economy; and we are seeking for economies.

The only further change which I need mention here is a change in the manner of publication, so that the two existing Records shall be united and form merely two parts of one complete Record. The Recorders would prepare the manuscript as heretofore, observing, however, the restriction just noted. This would then be turned over to the common publishing firm which would use it for the systematic part of the Record, and the manuscript coming from Naples for the morphological part. There is no reason why the name *Zoological Record* should not be kept for at least the English purchasers. The

form of the old Record might also be retained, even the present form of binding if desired. In short, the continuity would be perfect. The *Zoological Record* would be continued (as a *part*, of course), it would be left in the hands of the Zoological Society, and it would be greatly improved.

There remains then merely the question of expense. Here we have to my mind the most convincing argument of all. By the simple exclusion of Morphology, an economy would be attained; by having the *Record* published together with the *Morphological Record* a further saving of at least £100 would be made; by raising the standard of excellence the circulation ought to be increased; by the selection of a publisher with wide international connections in the zoological world the circulation could be increased; by federation with the Central Bureau, and its extensive organisation, it is believed that the circulation would be greatly increased. In short, I have the conviction that the annual deficit would become entirely insignificant. This fact may be doubted. To this I have a perfect answer. We are willing to assume the entire responsibility of the publication made under the conditions specified. Let the Bureau receive the manuscript ready for press, and for this privilege pay a fixed sum guaranteed by the publisher. I have conducted negotiations with publishers on this point; but for the moment it is only necessary to state that the contribution towards the support of the Bibliographer could be levied upon this sum, and yet that the amount which the Zoological Society would need to add in order to pay the Recorders would be *less* than the present deficit of £350. The entire reform could be carried out with an actual saving of money! I need add no further commentary. The Naples *Jahresbericht* is willing to co-operate. Many people sincerely hope that the *Zoological Record* will do the same.

HERBERT HAVILAND FIELD.

SOME NEW BOOKS.

Pocket Gophers.

Monographic Revision of the Pocket Gophers, Family Geomyidæ (exclusive of the species of *Thomomys*). By Dr. C. Hart Merriam. North American Fauna, no. 8; Published by the U.S. Department of Agriculture, Washington. 8vo. Pp. 258, with 19 plates, 4 maps, and frontispiece. 1895.

This valuable monograph is one of the results of the new methods of collecting and working at Mammalia now universal in America. These methods were originally introduced by the author of the present work, and by his energy and remarkable powers of field- as well as cabinet-work, he has revolutionised the whole study of Mammals in that country, and has shown to what a degree of refinement the science of Mammalogy may be brought. Starting as a private collector and observer of animals' habits, he soon perceived the great ignorance of the exact characters and distribution of small mammals which then prevailed, and set himself to remove it by organising a proper system of collecting. Of this system the essential points were that large numbers of specimens should be collected, that these should all be made up exactly like each other on a regular plan, that each should have its skull attached, and that the measurements of every individual taken in the flesh should be recorded.

Hardly had Dr. Merriam commenced work on these lines than he was appointed Chief of the United States Department of Agriculture, in whose province the study of small mammals (sad to say, mostly noxious) naturally falls, and by the energetic use of his widened powers he has accumulated in the Department, as a supplementary collection to that of the United States National Museum, a series of some 50,000 skins of North American mammals—a number perfectly incredible to naturalists of the old type, by whom one or two examples from each of a few widely-separated localities were generally thought to be sufficient to exemplify the range and characters of a species.

Turning, for example, to the Pouched Gophers included in this monograph, *Geomys* in its old sense, we should be very much surprised if all the European Museums together contain sixty specimens (our own National Museum has thirty-four), while Dr. Merriam in writing his work has had for study "upwards of 800 specimens belonging to the U.S. Department of Agriculture, supplemented by 110 specimens from my private collection, and a number from the U.S. National Museum, making a total of about a thousand specimens, among which are by far the greater number of types known to be extant."

The book itself (for though called a paper it is really a book) consists roughly of two parts, one general and the other systematic. In the first a full account is given of the Geomyidæ in general, their structure and habits, the descriptions of these latter being based partly on field notes and partly on observations made on a living specimen kept in captivity by the author.

Probably many or most readers of NATURAL SCIENCE are aware that the Rodents treated of have deep pouches in their cheeks, opening externally, but no one has hitherto known exactly how these pouches were utilised. Dr. Merriam tells us that they are for carrying food into store, and that the bits of potato, apple, or what-not are pushed into the pouch with the long claws of one fore-paw, while the other holds the pouch open, and that they are emptied again by being compressed by the paws against the cheeks from behind forwards, so that their contents are squeezed out of them on to the ground in front of the animal's mouth.

Another point of interest is the use of the tail. Gophers habitually run backwards along their burrows almost as freely as forwards. In so doing they use the short and nearly naked tail as a tactile organ to guide their footsteps, a use, so far as is recorded, quite unique in the Mammalia, although from its suitable length one might suspect that moles' tails are similarly used.

In the structural portion we are given a very full account of the skull; the forms of its individual bones and their methods of ossification, and its changes of form with age.

But the most interesting point of this part of Dr. Merriam's book is the account of the teeth, and the discovery of the remarkable method in which the molars are strengthened with enamel-plates. In all other rootless-toothed rodents, so far as is known, the outer walls of the molars are completely invested with a continuous coat of enamel, of approximately equal thickness all round, however much it may be grooved or infolded, and continuous on all sides equally with the original enamel-cap of the unworn tooth; but in *Geomys* the enamel investment of the top thins out and disappears on the sides at certain points, so that when the connecting-cap has been worn off the top, which takes place at a very early age, the enamel is merely present in vertical strips or plates unconnected with each other, and the positions of these are treated by Dr. Merriam as of considerable systematic importance. At the same time, it should be noted that out of the small number of skulls available for examination in this country we have found two marked exceptions to Dr. Merriam's enamel-plate characters, and are, therefore, surprised to find no mention of any variability, in this respect, among the enormously larger series examined by him.

In the systematic part of the work old-fashioned naturalists will find more to object to, mainly in regard to the extreme generic and specific splitting employed. For of the one old genus *Geomys* Dr. Merriam now makes nine, partly on the characters of the enamel-plates, and partly on those observable within the brain case, a region somewhat awkward to examine for those who have not material sufficient to justify them in cutting open a skull of each species, although, of course, this is no argument against the soundness of the conclusions arrived at. Of species and subspecies, thirty-seven are admitted, no less than twenty-one being new, numbers which contrast somewhat strikingly with the seven recognised by Baird in 1857, these even having been reduced to five by Coues in 1877. Still, it must not be supposed that any considerable proportions of the thirty-seven species are mere "splits" of those previously known, as a most astonishing number of genuinely distinct forms have been discovered by recent collectors in Mexico, and mainly by Mr. E. W. Nelson, of the Department of Agriculture, during his exploration of Mexico in 1892 and 1893.

The book, as a whole, is exceedingly well illustrated, the nineteen

plates and seventy-one text-figures being, to our mind, as nearly perfect for their purpose as can be. For, while not equalling in artistic beauty certain German plates of a similar nature, such as those of J. D. L. Franz Wagner, yet they express to the eye the objects represented, which are for the most part skulls and teeth, more exactly than any figures that we ever remember to have seen, and Dr. Merriam and his artists are heartily to be congratulated on them.

Of the very few misprints we have noticed, we may venture to point out one which, occurring in the synopsis of genera, may easily cause confusion to workers. This is that the top line of p. 24 should be transposed to four lines lower, below the diagnostic character of *Zygogeomys*.

Altogether, apart from the splitting, which will undoubtedly be considered too extreme by the majority of Old World naturalists, we do not hesitate to assert that this is one of the finest and most careful monographs of a mammalian group that has ever appeared, and we trust it may be followed by many like it, thus properly utilising the magnificent material contained in the department from which it has issued. O. T.

A Primer of Evolution.

A Primer of Evolution. By Edward Clodd. 8vo. Pp. 186. London: Longmans, Green & Co. 1895. Price 1s.

This little book has an importance out of proportion to its size, for it is designed to bring the "comfortable word evolution" home to the great heart of the people. While the "Origin of Species" has its tens of thousands, Mr. Clodd's readers will be a great crowd, innumerable as the sands of the sea-shore. For such a book, a special standard of criticism is necessary. It were fruitless to object to the hypothetical inclusion among organisms of the discredited *Eözoon canadense*, to the practical neglect of the wonderful series of American beds that represent the gap that occurs in Europe, between the end of the Chalk period and the beginning of the Tertiary strata, or to the identification of the cavity in Hydra-like polyps with the body-cavity of higher animals. Mr. Clodd's readers have no examinations to pass; it is the general and not the particular for which they are ahungered. Each one of us, coming into the world and struggling into a tiny place, sometimes must forget the perpetual personal conflict in larger questions. It may be the wandering wind, or the silent stars, or the repulsive profusion of life under an up-turned stone that sets us thinking. What are we? What is the place of our littleness in the greatness of the universe?

For such questioners Mr. Clodd provides what answers are possible. He shows the universe as an ordered system of change, of evolution and devolution. "The universe is made up of matter and motion, or, to put this in other words, the universe is made up of distinct particles which are never still, whether they compose things living or dead." . . . "Nebulæ, circular, elliptical, spiral are the raw material of which suns and systems are formed. The nebulæ; the fixed stars, with whatever belongs to the system of each; the wandering comets; and the myriad meteor streams . . . comprise the ponderable matter of the universe." . . . "The star nearest to us is known as the sun, a mass of gas burning at so high a temperature that all the chemical elements of which it is composed are in a state of vapour." With the sun comes his system of planets,

which, "like the stars, are in different stages of progress and decay. Some, as the Earth and Venus, at least in her Polar Regions, have cooled down sufficiently to be covered by a hard crust, and to be fit abodes for living creatures; others, like Jupiter and his fellow giant planets, are still in a more or less heated and partly self-luminous condition."

Having, so to say, put the earth in her place, Mr. Clodd proceeds to give a brief review of her geological history and of the fossil record of gradually evolving forms of life. Then with a pleasant lucidity he describes the gradations that exist among the existing forms of animals and plants.

The second section of his book Mr. Clodd entitles "explanatory." In that, positing matter and force and the unequal distribution of the masses of matter in a first chaos of the universe, he describes the origin of stars and systems, and in particular the origin of our own planetary system. Then he gives a brief review of the evolution of organic life on our own globe, and of the arguments supporting Darwin's principle that Natural Selection is the chief agency in this. All this part is done admirably and clearly, in a fashion that will command the respect of those familiar with the difficulty of presenting so complicated a series of arguments in a compact and coherent form.

Mr. Clodd has done his work well, and the inquiring reader will find, not indeed an answer to the problem of the universe, but something on which to stay his mind. The acceptance of the principle of evolution as ruling all the changes of matter and force, whether they be displayed in the great masses of stellar space or in the surging changes of the atoms and molecules of organic matter, merely pushes back the "why" of things to a further place. But the grandeur and the universality of the "explanation" afforded by evolution may narcotise many minds into peace; and when there are no ultimate explanations one narcotic is as good as another.

DR. HERTWIG'S TEXT-BOOK OF ZOOLOGY.

LEHRBUCH DER ZOOLOGIE. By Dr. Richard Hertwig. Third improved edition. Pp. 600, many illustrations. Jena: Gustaf Fischer, 1895. Price 11 marks 50 pf.

THE third edition of Dr. Hertwig's text-book has been carefully revised, and a considerable amount of new material introduced. The account of the Porifera has been much modified, a very large addition to our knowledge of this group having been made since the earlier edition appeared. The Lamellibranchs, too, appear in an altered form, as recent work of Neumayr, Pelseneer, Jackson, Grobben, and others make our former views untenable. The palæontological side has been slightly extended, but it still remains the weakest side of the book. In many respects we regard Dr. Hertwig's text-book as considerably in advance of other books in English, German, or French, and we think it the more to be regretted that due importance is not given to extinct forms. The divorce between palæontology and zoology, so lamentable in museums, is even more lamentable in text-books. Where the zoologist ventures upon palæontology he is always insufficient, and not infrequently erroneous. Where the palæontologist ventures upon zoology, so far as we have had opportunity of judging, he is almost invariably ludicrous. A true text-book of zoology remains to be written; but of those in existence Dr. Hertwig's, in lucidity and correctness, is probably the first, certainly among the first three.

Birds and Books.

Summer Studies of Birds and Books. By W. Warde Fowler. 8vo. Pp. 288. London: Macmillan & Co. Price 6s.

A series of unconnected but interesting essays are brought together into this volume from various tributary sources. Articles from magazines, papers read before natural history societies, and one separate publication in pamphlet form constitute the eleven chapters of which Mr. Warde Fowler's most recent book is made up. They are all worth reading, some of them particularly readable. Mr. Fowler dips deeper into his subject than do some ornithologists, and emerges with many valuable and not easily accessible facts. These are the result, not only of personal observation, but also of literary research. As an Oxford man, Mr. Warde Fowler is naturally a student of Aristotle; and he presents us with a highly interesting sketch of Aristotle as an ornithologist, a character in which he probably is not familiar to Mr. Fowler's colleagues of the University. It appears from what we are told that Aristotle was not altogether irreproachable as an ornithologist. His statements are not entirely the result of observation, and, as a consequence, are constantly erroneous. But mingled with much error are a few pearls of some price; it is interesting, for instance, to learn, rather from what is left unsaid than from what is said, that in the days of the father of natural history the House Martin had not yet acquired that mode of nesting which has given to it its name. His observations upon the Nightingale teaching its young to sing have been since confirmed. And Aristotle knew three centuries before Christ, "what we moderns only learnt a century ago, that a bird does not sing its own song by instinct, but will sing another bird's song if the proper chance is given it." One of Aristotle's weak points appears to have been colour, a defect which, according to certain authorities, he shared with no less a person than Homer. "Aristotle is often content with telling us that a bird is of a bad colour or a good colour without troubling himself further, as if he well knew that his countrymen were not gifted with an acute colour-sense." Besides, as Mr. Warde Fowler remarks, the Greeks were not by any means amply provided with words for the different colours, let alone diverse shades of a given colour.

Strongly though Mr. Fowler speaks in praise of Aristotle, he has plenty of commendation left for Gilbert White, to whose life and merits another excellent essay is devoted. In these days of unceasing "vorläufige Mittheilungen" and disjointed publication, it is good to read how Mr. Fowler emphasizes the fact that White did not print his famous book until he was close upon seventy. On the other hand, though the author particularly points out that his hero was "White of Selborne, not White of Oxford" he, obviously, came perilously near to being as barren (intellectually) as the average Oxford Don; for he died only four years after the book was published. We fear very much that the "ease and isolation, the complete absence of hurry and worry" to which Mr. Fowler attributes the greatness of the Natural History of Selborne, will soon cease to be attainable. Brixton and Clapham prefer something more useful than a "good monography of worms"; while the author of such a monograph would not be, in the opinion of a certain politician, one of those men who are worth more than five hundred a year.

The remaining essays of Mr. Fowler's volume are nearly all the results of direct observation upon bird-life, while an appendix, partly contributed by Mr. Pycraft, of Oxford, helps to elucidate a chapter upon the song of birds. F. E. B.

DORSET BOTANY.

THE FLORA OF DORSETSHIRE, with a Sketch of the Topography, River System, and Geology of the County. By J. C. Mansel-Pleydell, B.A., F.G.S., etc. Second edition, with two maps. 8vo. Pp. 345 and 63. Dorchester: *Dorset County Chronicle* Printing Works, 1895.

WE are glad to see that Mr. Mansel-Pleydell has brought out a second edition of his "Flora of Dorsetshire," in which is incorporated the additional knowledge gained since 1874, the date of the first edition. Though "printed for private circulation only," this valuable work will doubtless find its way into the hands of all interested in the botany of Dorset in particular and of Great Britain in general. The flora is prefaced by a useful sketch of the topography and geology of the county, and there are also two very good maps, geological and botanical respectively. Under each species the author cites the first record, the habitat of the plant, the localities in which it occurs, and its general distribution as regards the neighbouring counties of Devon, Hants, Somerset, and Wiltshire, and also Normandy. With vascular Cryptogams and Characeæ, there are 1,173 species; this includes, however, certain denizens and aliens.

The fair number of records credited to his own name shows that Mr. Mansel-Pleydell has himself laboured in the field, as well as entered into the labour of others, the latter, indeed, with full acknowledgment.

INJURIOUS INSECTS.

REPORT OF OBSERVATIONS OF INJURIOUS INSECTS AND COMMON FARM PESTS, during the year 1894, with Methods of Prevention and Remedy. Eighteenth Report. By Eleanor A. Ormerod. 8vo. Pp. viii., 124, lxii., 4. London: Simpkin, Marshall & Co., 1895. Price 1s. 6d.

THIS is a book distinctly aggressive to insects. It is most interesting to the general reader, and invaluable to the farmer. It would be instructive to know how many farmers buy it, and then how many of the buyers put the valuable advice it contains into practice.

In a short preface, Miss Ormerod says "the most important discovery of the year is that which we owe to the skilled researches of Professor John Percival, of the South Eastern Agricultural College, Wye, near Ashford, Kent, of the presence of two kinds of eelworms at the roots of Hops, of which one kind, which is seriously destructive to various kinds of crops on the Continent, had not previously come under notice in this country, and neither had been previously observed at Hops." A full account of Professor Percival's discovery has been given by Miss Ormerod in her Report (pp. 47–61). She understood that it was to have been published in the *Journal of the Royal Agricultural Society* for 1894. As a matter of fact, that Society did not publish the paper at all, but it was published in NATURAL SCIENCE for March this year, and Miss Ormerod has acknowledged the unexpected alteration in a slip inserted in her Report.

The insects which receive honourable mention in Miss Ormerod's present Report are only two in number,—the Golden Eye (*Chrysopa perla*), and the Eyed Ladybird Beetle (*Coccinella ocellata*). The first of these, known also as the Lace-wing Fly, is carnivorous in the grub stage, and will eat aphides, its fellows, or a caterpillar much larger than itself; the second, the "ladybird," has a partiality for Red Spiders, and while in the maggot state devours them voraciously. All other insects mentioned in the Report are condemned,

and the best methods for their destruction fully set forth. One of the most destructive insects of the past year was the "Antler," or "Grass" Moth (*Charæas graminis*, Stephens; *Cerapteryx*, Curtis), which appeared in great numbers in the South-West of Scotland, seven counties being affected. So bad, indeed, was the plague that Professor Wallace remarked early in July, "they are said to be worse than the voles in some places." Nature supplied a remedy, however; for the caterpillars were attacked by a threadworm of the genus *Mermis*, and by the disease known as "flacherie."

Perhaps the most interesting part of the whole Report is the appendix of 62 pages, devoted entirely to "Observations on Warble Fly or Ox Bot-Fly" (*Œstrus bovis*, Linn.; *Hypoderma*, De Geer). Miss Ormerod has collected a great amount of additional information together regarding this pest,[1] which, besides causing serious pain and distress or even death to the animal, renders its skin, in bad cases, quite unfit for sale purposes. A picture of a piece of the skin of a yearling, 24 by 14 inches is given, which shows 402 warble holes, and this will give the reader some idea of the commercial value of such a hide for leather. Full particulars are given of the condition, value, and depreciation of the hides of oxen attacked by warble, and if this commercial point could be forced upon the farmer, methods might be adopted which would relieve the cattle, and, at the same time, increase the value of his property. The remedies suggested and approved of by practical men are large shallow ponds, where the animals can stand during the heat of the day, trees, and careful housing at night, smearing the back with Stockholm or green tar, brine, sulphur, train-oil, McDougall's Smear, or some other strong-smelling or fatty matter. If the beast is warbled the grub must be extracted by pressure after a small incision with a penknife.

We might quote many pages from this interesting Report, but advise our readers to get it and read it for themselves; they will then, perhaps, realise the debt of the agriculturist to Miss Ormerod, and the scope of the good work that she is doing.

The United States is, however, the true home of economic entomology, and we have frequently called the attention of our readers to the excellent reports issued by the various institutions of that country. The latest of these reports received by us is the eighteenth of the State Entomologist on the Noxious and Beneficial Insects of the State of Illinois, which contains a monograph of "Insect Injuries to Indian Corn," part 1, by Mr. S. A. Forbes, the State Entomologist. This deals with classification of injuries, injuries to the seed in the earth, injuries by ants, beetles, and the special insects that affect the seed, injuries to the roots, with the special insects that inflict them. The majority of these pests are figured, and many of them described for the first time. Fifteen good plates accompany the monograph, which is well and fully indexed, and which cannot fail to be of the greatest service to growers in America and elsewhere.

A Bold Undertaking.

The Great Problem of Substance and its Attributes, involving the Relationship and laws of Matter and of Mind as the Phenomena of the World, derived from the Absolute. 8vo. Pp. 197. London: Kegan Paul, 1895.

In this little book an attempt is made to solve the problem of the universe in terms of Ether regarded as absolute substance. We are

[1] See Reports for 1884, 5, 6, 7, 8, 9 and 90.

told, to begin with, that the luminiferous ether radiates or carries forth the forms and conditions of external objects from the objects to the external organ of perception. Thus we are introduced to the ether as mediating between the perceived object and the perceiving subject. It is then assumed that this ether is the absolute substance, and that it is to be regarded as peculiarly of the nature of spirit. Furthermore, it has the capacity of "taking on and setting forth in its own bosom what we may designate spirit copies of all material objects." But, as absolute, it must constitute the material objects themselves. To the question how the qualities of the ether can become the foundations of matter, the author replies, By the process of thickening. "How the spirit form is converted into a material form we have no means of showing further than this, that there is a process of incrassation." The ether thus becomes both the object and the medium by which it is revealed to the subject-mind. What, then, is the nature of this subject-mind? In answer to this question we are told that "there is much, very, very much, to lead us to believe that the operation of mind is neither more nor less than our old friend in a new position—the ether, found as the spirit medium in physics, and always working in and along with matter." Having now got the ether "incrassated" as matter, the ether "in a new position" as mind, and the ether as spirit medium to mediate between itself as "incrassated" and itself "in a new position," we may regard our solution of the problem of the universe with such complacency as it is in us (not being the author thereof) to summon up.

It is obvious that we cannot, in NATURAL SCIENCE, do more than barely indicate the nature of the author's thesis. One example of the kind of use to which the principles reached are put must suffice. "The ethereal representation of the sun," we are told, "meets the ethereal representation of the earth, and that of the other planets of the solar system, and thereby maintaining the centripetal relation of each to the sun, while their movement given to each centrifugally balances this relation into a definite circumambience." The illumination thrown upon other matters of physics and of psychology is of like nature.

C. LL. M.

GEOLOGY OF SWEDEN.

SVERIGES GEOLOGI ALLMÄNFATTLIGT FRAMSTÄLLD MED EN INLEDANDE HISTORIK OM DEN GEOLOGISKA FORSKNINGEN I SVERIGE JEMTE EN KORT ÖFVERSIGT AF DE GEOLOGISKA SYSTEMEN. By A. G. Nathorst. 8vo. Pp. 336, with several hundred illustrations. 1892–94. Stockholm: F. & G. Beijers. Price 8 kronor [9s.].

THE mass of geological literature is now so overwhelming, that everyone will be thankful to an author who gives a clear account of the geology of his own country within a reasonable number of pages. But when the writer is also the person best qualified to deal with the subject, and is in the front rank of original observers, our obligation, and our confidence, become still greater. On looking through Professor Nathorst's handsome volume we find that he has done for Sweden what has been done for few other countries. He has written a geology, or we may, perhaps, better describe it as a geological textbook, in which the greater number of pages is devoted to the formations and types of deposit represented in his own land. Formations absent in that country are only mentioned in the introductory sketch (pp. 20–38), and if good material is available the author always figures

by preference Swedish specimens. This makes the book of great value for comparative studies, and we should advise English geologists, even if they do not read Swedish, at least to examine the illustrations and tables, for the volume is full of original or otherwise inaccessible material. We might particularly point to the chapters relating to the Cambro-Silurian, Jurassic, Cretaceous, and Quaternary, and to the illustrations and remarks about fossil plants throughout. It should be remembered that Professor Nathorst, though well-known as a field-geologist, is almost more celebrated as a student of fossil plants. So far as we can call to mind, no previous geological manual has been written by a botanist, and the botanical evidence, therefore, has been generally overlooked or misinterpreted. The book is provided with an exhaustive index, and a classified bibliography of nine closely-printed pages.

Dr. Ch. Janet has sent us a series of papers on ants, published in the *Comptes Rendus, Mémoires de la Société Zoologique de France*, and *Mémoires de la Société Académique de l'Oise*. Two other papers, from the *Comptes Rendus*, will be of some interest to those who, like our contributor, Mr. Latter, study the habits of wasps, and their resistance to unfavourable weather. They deal with the construction of the nest, the order of formation of its cells, and the preservation of heat within it. This last is a point of much importance, for if the temperature be reduced, the development of the eggs may be indefinitely arrested.

Another paper from Dr. Janet appeals to a very different class of readers, namely, the collectors and students of fossils. The Tertiary Bracheux Sands contain beautiful fossils, but, unfortunately, these are exceedingly friable. Dr. Janet shows how, by imitating a natural process, this powdery limestone may be transformed into solid gypsum, and the specimens extracted with safety. A strong solution of sulphate of lime, slightly acidified with sulphuric acid, is gently poured over the fossiliferous sand, thus the fossil is transformed into sulphate of lime without being in any way dissolved, and without being ruptured by the escape of gas. The note is contained in *Comptes Rendus de la Société Géologique de France*.

We understand that Messrs. Macmillan will publish early in May a work upon Birds by Mr. F. W. Headley. It will consist of some four hundred pages, octavo, with abundant illustrations, largely in the form of plates. The book will be suitable for those who are not experts in bird anatomy, though its treatment is strictly scientific. Great weight is laid upon the anatomy of the organs of flight; but such general topics as song and colour are dealt with also, though not at such great length.

The Clarendon Press will shortly issue a "Monograph of the Oligochæta" by Mr. F. E. Beddard, F.R.S. This is to be a quarto volume of nearly 800 pages, with five plates and a number of woodcuts. Our knowledge of the structure and of the species of the terrestrial worms has increased so largely of late that a general treatise embodying what is known upon the subject is much to be desired.

OBITUARY.

JAMES DWIGHT DANA.

BORN FEBRUARY 12, 1813. DIED APRIL 15, 1895.

AMERICA has to deplore the loss of one of the greatest of her sons in the death of this eminent zoologist, mineralogist, and geologist, which occurred at New York on April 15. James Dwight Dana was born at Utica, New York, February 12, 1813. He entered Yale College, where he graduated in 1833, paying especial attention to the natural sciences and mathematics. His first position was that of teacher of mathematics to midshipmen in the United States navy, and in the course of his work he visited the Mediterranean. Returning to America in 1835 he became assistant to Benjamin Silliman, whom he ultimately succeeded in the chair of Mineralogy at Yale. So early as 1837, Dana published his "System of Mineralogy," a work which has run through many editions, and is undoubtedly the first on the subject. The sixth and last edition was published in 1892, and in this he was largely assisted by his son. In 1836 he was fortunate enough to be appointed Mineralogist and Geologist to the Wilkes' Exploring Expedition to the Southern and Pacific Oceans. This expedition sailed in 1838 and returned in 1843, and for the next thirteen years Dana was engaged in editing the various reports sent in for publication, and himself contributed the reports on Geology, on Crustacea, and on Zoophytes, making, with few exceptions, the drawings of the animals himself. In 1850 he was elected as Professor Silliman's successor; but it was not till five years later that he took up the work. In 1854 he was President of the American Association for the Advancement of Science, his annual address being delivered at Providence in 1855. In 1863 the first edition of his "Manual of Geology" appeared, and this book has attained, so far as American geology is concerned, a position equal to that of his "System of Mineralogy"; only this year (1895) a new edition has been published, and the manuscript for this, we are informed, was in the veteran Professor's own handwriting.

"Corals and Coral Islands," another well-known book, appeared in 1872, while his "Characteristics of Volcanoes" did not see the light till 1890. His contributions to serial and academical literature were very numerous, and since 1846 he had been an editor of the "American Journal of Science," the best known of scientific serials of the United States, which was founded by the elder Silliman in 1818. He resigned his Yale Professorship in 1894.

Professor Dana was a Member of the Academy of Sciences of Paris, a Fellow of the Royal Society, of the Academies of Berlin and Munich. He received the Copley Medal in 1877, and the Wollaston Medal of the Geological Society in 1872.

As a naturalist, Professor Dana's work was characterised by the grip of a master-mind. In the course of his long career he had to undertake many very different and difficult tasks. In each of them the same mental grasp of the principles of the subject was shown. His great report on the Corals of the Wilkes' Expedition is the most indispensable monograph on the Reef Corals. His classification of the Crustacea, though artificial, is still used by zoologists. His work on mineralogy was a masterly synopsis of the knowledge of the science. His contributions to geology profoundly influenced contemporary thought on such problems as the formation of coral-islands, the permanence of oceans and continents, and the characters of volcanic action. In each of these Dana's mastery of his subject and his powers of careful, detailed observation were exhibited. His work was done to last. In his soundness of judgment, his wide range of knowledge, his thoroughness in research, he ranks intellectually with the great legal codifiers rather than the naturalists. In his works on coral-islands, for example, he stated the contending theories with judicial fairness and lucidity. He gave his verdict in favour of Darwin's views, and added some original arguments of great importance in their support. But he was inferior to Darwin in imagination. His classifications were artificial, and his views were more often in error, owing to excessive caution and lack of daring than from any other failing. Thus his masterly paper on the submerged fiords at the mouth of the Hudson River exercised only a very small proportion of the influence it might have had if he had not thrown back the formation of the valley to the Trias, apparently because his imagination recoiled from the conclusions that followed from attributing it to a later date.

But caution is probably an error on the right side, especially in a man who worked on so many subjects and on so many lines as did Dana. His shorter papers, including several contributions to NATURAL SCIENCE, were very numerous and extraordinarily diverse in their range. It is however as a great monographer that he will be best remembered, and it is his great systematic reports on corals, crustacea, vulcanity, and mineralogy that will form the most enduring monument of his fame.

JOHN ADAM RYDER.

BORN 1852. DIED MARCH 26, 1895.

THE death of this distinguished American embryologist will come as a great shock to his many English friends. Professor Ryder was born near London, in Franklin Co., Pennsylvania, and received

an ordinary school education; but his progress was interrupted by financial trouble in the family. He became a student at the Academy of Natural Sciences of Philadelphia under the Jessup Fund endowment in 1874, and the fund was renewed to him from time to time in recognition of his "extraordinary singleness of purpose, and his ability as an investigator." Indeed, he held the fund until March, 1880, when he entered the service of the United States Fish Commission at the instance of Professor Spencer Baird. From 1880 to 1883 he investigated the development, habits, and breeding-grounds of the mollusca and fishes, and his results have been published in a long series of papers in the Bulletins of the Commission. He was a particular authority on oyster culture, and the artificial propagation of the salmon and the sturgeon. In 1886 he became Professor of Comparative Embryology at the University of Pennsylvania, and, though actively engaged in teaching, was never idle with his pen. The great majority of his writings, outside the work done for the Fishery Board, were published in the *Proceedings* of the Academy of Natural Sciences of Philadelphia, of which he became a member in 1878; but the American Philosophical Society, the *American Naturalist*, and numerous European journals were enriched by his communications. Ryder was a great opponent of Weismannism, believing that all phenomena of living organisms could be explained by mechanical laws. He possessed a wonderful memory, and had a strong inventive genius, as witness his microtome, now so extensively used. His last two papers on "An arrangement of the retinal cells in the eyes of fishes, partially simulating compound eyes," and on "The true nature of the so-called 'nettle-threads'" are in the hands of the Philadelphia Academy.

TOYOKITSI HARADA.

WE regret to learn of the death of the eminent Vice-Director of the Geological Survey of Japan. Dr. Harada was well-known in Germany, where he spent a great part of his youth, studying at Harburg, Freiburg i/S., Heidelberg, Munich, and Vienna. In the summer of 1880 he joined the Austrian Geological Survey as a volunteer, and in 1881 began his independent researches with a study of the eruptive rocks near Lake Lugano, which formed his doctoral thesis at the University of Munich. During the summer of 1882 he mapped a part of Comelico and Western Carnia, in the Venetian Alps, and published an account of the geology (*Verh.* and *Jahrb. k. k. Geol. Reichs.*). In 1883, after an extended journey through middle and south Europe, he returned to his native land, where he soon succeeded Dr. E. Naumann as inspector of the geological and topographical surveys. His more important works in Japanese geology are "Versuch einer tektonischen Gliederung der Japanischen Inseln" (*Tokio*, 1888), and "Die Japanischen Inseln" (*Berlin*, 1890), a work which is still unfinished. Of the official work which he so

largely organised and directed, a full account was given in our pages a year ago. Dr. Harada had for some time been in failing health; he died on December 1, 1894, and his funeral was attended by many Europeans, and especially by members of the German colony in Tokio.

ALEXANDER GOODMAN MORE.

BORN 1830. DIED MARCH 22, 1895.

IRISH science has sustained a severe loss in the death of this accomplished and genial naturalist. Of Scottish extraction, and educated at Rugby, Cambridge, and Switzerland, Mr. More, after some valuable field-work in the South of England, settled in Ireland about 1860. Our knowledge of the flora and fauna of that country has been vastly increased by his labours. In 1866 he published, in conjunction with the late Dr. D. Moore, a "Cybele Hibernica," giving a summary of the distribution of plants in Ireland, of the greatest value, not only for the accuracy of the author's own facts, but for the careful sifting of evidence with regard to doubtful records. It is a matter for the deepest regret to Irish botanists that the second edition of this work, for which material had been assiduously collected by Mr. More almost up to the last, can now never be published by him. In 1867, More became assistant in the Dublin Natural History Museum, of which institution he was appointed Curator in 1881. In his zoological work he devoted most of his attention to birds, and issued, under the Museum auspices, a valuable list of the Birds of Ireland. His knowledge of other vertebrate groups was, however, very considerable, and he was also well acquainted with the Mollusca and Lepidoptera. Both in botany and zoology, Mr. More excelled as a systematist and field-worker. He made no claim to be a morphologist or anatomist. A lamentable accident and lengthened illness forced him to retire from the Dublin Museum in 1887. Since then, debarred from his well-loved outdoor studies, he has cheerfully borne much suffering, while taking a kindly interest in the work of younger men, who have always found him eager to unlock his stores of practical knowledge for their benefit.

EDWARD HAMILTON ACTON.

BORN 1862. DIED 1895.

THE sudden and premature death of Edward Hamilton Acton at St. John's College, Cambridge, on February 15, has deprived the University of an able and vigorous worker in the Natural Science School. Born at Wrexham in 1862, Acton was educated at Rugby and St. John's College; after obtaining a first-class in the second part of the Natural Sciences Tripos in 1885, he was elected Fellow of his College in 1888. For the last seven years he devoted himself chiefly to chemical teaching, and was recently appointed College Lecturer in

that subject. In his chosen field of reseach, botanical chemistry, he had already proved himself an investigator of considerable ability. His last work on this subject is contained in the recently published "Practical Physiology of Plants," the authorship of which he shared with Mr. Francis Darwin. A perusal of the papers contributed by Acton to botanical science does not lead to an adequate appreciation of his scientific attainments; but those who were privileged to be his colleagues know too well the severe loss which botanical chemistry has sustained. As a friend has admirably expressed it, Acton was in truth "a man"; a teacher who won the best form of appreciation, the respect and friendship of his students, and a friend who will always be remembered by those who knew him, not only as an able scientific thinker, but as one in whose strong personality there was reflected a sterling manhood, a thoroughness of action, and an honesty of purpose.

ROBERT FITCH.

BORN OCTOBER 21, 1802. DIED APRIL 4, 1895.

THIS well-known Norfolk antiquary and geologist was born at Ipswich, and was educated at the grammar school of his native town. He was apprenticed to a chemist and druggist, removing for a few years to London, and in 1827 entering into partnership with his brother-in-law at Norwich, where he has ever since resided. He became a Fellow of the Geological Society in 1844, and devoted his energies in a geological direction to the collection of flint implements and vertebrate remains in particular. These remains, together with an extensive and valuable series of antiquarian interest, he presented in 1892 to the Norwich Museum, himself providing the cases in which they are displayed. Fitch's writings were mainly concerned with archæology, but his contributions to geology were of considerable value and interest. He numbered among his friends Sedgwick, Murchison, Darwin and Lyell, and was in constant correspondence with men of the younger school, one of whom, Mr. E. T. Newton, fittingly attached his name to a new *Cervus* found by him in the Forest Bed at Bacton.

Robert Fitch was a magistrate for the city of Norwich from 1858, he became sheriff in 1867, was a director of the Norwich Union Fire Office, and was a most important man to his town. He was one of the last of the old Norfolk geologists, who numbered among them such names as Samuel Woodward, R. C. Taylor, J. W. Robberds, C. Green, Anna Gurney, and a host of others of more or less importance.

JEAN EDOUARD BOMMER, Professor in the Brussels University, Curator of the State Botanic Garden, and formerly President of the Royal Botanic Society of Belgium, died at Brussels on February 19, in his 66th year.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

MR. CHRISTOPHER HEATH has been elected President of the Royal College of Surgeons in the room of the late Mr. Hulke. Dr. Patrick Manson has been appointed Lecturer on Tropical Diseases at St. George's Hospital. This hospital is the first institution, so far as we know, which has provided for special instruction in tropical medicine in London. Professor C. S. Sherrington, late Fellow of Gonville and Caius College, Cambridge, has been appointed to succeed Professor Gotch in the George Holt Chair of Physiology at the University College, Liverpool; Professor E. L. Greene succeeds to the Botanical Chair in the Catholic University of Washington; Mr. T. T. Groom has succeeded the late Professor Allen Harker as Professor of Natural History to the Royal Agricultural College, Cirencester; Mr. J. C. Sumner has been appointed by the Liverpool Marine Biological Committee to be Curator of the Port Erin Station; Mr. J. Percy Moore is taking temporary charge of the late Professor J. A. Ryder's classes at the University of Pennsylvania.

SIR JOSEPH LISTER received at the hands of the Prince of Wales, on the 9th inst., the Albert Medal, awarded to him by the Society of Arts for "The discovery and establishment of the antiseptic method of treating wounds and injuries, by which not only has the art of surgery been greatly promoted and human life saved in all parts of the world, but extensive industries have also been created for the supply of materials required for carrying the treatment into effect."

MR. JOHN WARD, of Longton, has been awarded the Robert Garner Medal by the North Staffordshire Field Club for his researches into the fossil fauna of his county. Mr. Ward's fine collection of Coal-measure fishes has been added to the national collections, and he is now working up the molluscan fauna of the same series of rocks.

WE learn from *Nature* that the Paris Geographical Society has awarded its prizes for 1895, as follows:—Gold medals, to Lieut. L. Mizon, for his explorations in West Africa; E. Gautier, for his explorations in Madagascar; F. Foureau, for his explorations in the Sahara; E. Ponel, for his explorations in the region of the French Congo; Th. Moureaux, for his magnetic map of France; Father Colin, for his observations and triangulations in Madagascar; A. Courtry, for the production of a map of the Congo; V. de la Blache, for his general atlas; and Dr. Thoroddsen, for his explorations in Iceland. Silver medals have been conferred upon E. D. Poncins, for his journey from Turkestan to Kashmir by the Pamirs; J. Gaultier, for his works on the production of plans by photography; B. d'Attanoux, for his exploration in the Sahara; and J. Forest, for his studies on the breeding and habits of the ostrich in the Sahara. The Jomard prize has been awarded to L. A. Rainaud, for his memoir entitled "Le Continent austral: hypothèse et découverte."

PROFESSOR JOHN MILNE had just resigned his appointment at Tokio, and was on the eve of his final return to England, when, on February 17, his house, his

valuable seismometric apparatus, and his accumulated notes of twenty-five years work were destroyed by fire in half-an-hour. Merely to state the fact is enough to arouse the warmest sympathy for our energetic and genial colleague.

DR. WILLIAM MARCET will deliver the Croonian Lectures at the Royal College of Physicians in June. He will take for his subject "The Respiration of Man."

THE annual meeting of the Zoological Society of London will be held on April 29. The Report, which has just been issued, shows a falling off to the extent of £1,300 in admission to the gardens, with a corresponding decrease of £90 in elephant rides, etc. The admission fees, subscriptions, and composition fees increased as compared with the year 1893. With regard to payments, on the other hand, there has been a considerable decrease in expenditure, more particularly noticeable because some heavy items, which properly belonged to 1892, were included in the year 1893. The whole account shows an excess of income over expenditure to the amount of £1,500, and this is carried forward to 1895. The Society paid £4,205 1s. 10d. for provisions for the animals during 1894, spent £1,008 on purchase of animals and carriage, £1,800 on publications (including the *Zoological Record*), and £500 on the library. On the income side of the account we read the following items of interest:—Riding receipts, £680; rent for refreshment-rooms, £762; garden sales, £522.

FROM the sixty-third Annual Report of the Royal Dublin Zoological Society for 1894, we learn that the attendance at the gardens was 10,000 more than in 1893. This is a surprising increase, and we congratulate the Society on the improvement which this has made in the finances. Nine lion cubs have been born, five of which were males in one litter. Three pumas were also born in the gardens. Great improvements have been made in the aquarium, and among the chief accessions are an orang-outang, a pair of ostriches, and a pair of South African hunting-dogs.

THE Royal Geographical Society will hold a special meeting on Monday, May 20, to commemorate the fiftieth anniversary of the departure of the Arctic Exploring Expedition under Sir John Franklin. In connection with this it is proposed to form a party to visit Greenwich on the same day, to inspect the Franklin relics in the Museum.

THE Museums Association will hold its sixth annual meeting in Newcastle-upon-Tyne, under the presidency of the Rev. Canon A. M. Norman, during the fourth week of July. The meeting will begin on July 23. The mornings will be devoted to the reading and discussion of papers, while in the afternoons the members will inspect the Museums and Art Galleries in the city. Inquiries should be addressed to Professor M. C. Potter, Durham College of Science, Newcastle-upon-Tyne.

THE sixth Annual Report of the Epsom College Natural History Society is just to hand. The list of members shows an increase of twenty-nine over that of the previous year. We regret to hear that the adjudicators of the Fayrer Prize did not receive an essay of sufficient merit to allow of the award. A number of excellent lectures were given, and good work has been done in the botanical and entomological sections in recording the finds of the year.

THE plaster cast of the *Iguanodon* from the Brussels Museum, which we referred to last month, has now been mounted and can be seen in the gallery of Fossil Reptiles in the British Museum. The growth of this portion of the collection is so large, and the important discoveries made in recent years so inadequately represented

for want of space, that it is to hoped some better accommodation for the class Reptilia will soon be made. The large series of interesting and remarkable dinosaurs from America are as yet known in this country only from pictures.

THE Natural History Society of Queensland, which was founded in 1892, has just issued its first volume of Transactions.

THE Danish Geological Society, which was founded January 16, 1893, has already published its *Meddelelser fra Dansk Geologiska Forening*. No. 2 of the *Meddelelser* consists of an important paper in Danish on the "Pleistocene Foraminifera of Denmark and Holstein" (Copenhagen, 1895, 230 pp., plate and map), by Victor Madsen. Dr. Madsen has collected from a very large number of localities, gives the results of his washings, and describes, in the latter pages of his paper, the foraminifera in detail. It is interesting to note that of his eighty-eight forms described, he only recognises one as new, a proof of the care the author has taken in identifying and working out his material. This new form is certainly curious; it is called *Lagena danica*, and consists of an oviform test, cut off at the greatest breadth by an oval margin; the base consists of a deep groove surrounded by a thick but flat margin, the whole structure reminding one of a thick flat india-rubber band seen sideways. Six examples have been found in three different localities. The paper permits of a valuable comparison of the Pleistocene foraminiferal fauna with that of the recent Baltic and North Seas.

THE sum of £500 has been bequeathed to the Geological Society of Cornwall by Mr. William Bolitho. The income derived from this money is to be applied each year to the gift of a medal "to be presented to the member of the Society whose attainments, labours, or discoveries in geological or mineralogical science are found most deserving."

DR. LOPEZ VIEIRA, of the University Museum of Coimbra, makes an appeal for the better development of Portuguese museums in *Annaes de Sciencias Naturaes* for January, 1895. He refers in particular to the museums of Washington, London, and New York, and the arrangements there in force with relation to the natural habits of birds and mammals. Dr. Vieira also notes the value to agriculture of the work done by the museums of the United States, an example which might well be followed in London, where systematic zoology alone seems to be in favour. The especial feature of agricultural value that Dr. Vieira quotes is an examination of the stomachs of 2,700 hawks and owls, in order to find out the particular nature of their ravages ("Hawks and Owls of the United States." By Merriam and Fisher. Washington, 1893).

A PARAGRAPH in our April number, on p. 286, made certain statements concerning the Museum at Colchester, which should properly have related to the Chelmsford Museum. The Colchester Museum is purely archæological, having recently been weeded of its few natural history specimens to make room for its fine collection of antiquities. It has never been closed, except for cleaning, since it was first opened some thirty years ago. The number of Essex newspapers that have offered us a prize of two guineas for our "extraordinary concatenation of journalistic blundering," shows an interesting ignorance of the English language and a lack of originality among our esteemed contemporaries, but so far has added nothing to our exchequer.

THE Zoological Society of France has re-issued the "Rules of Nomenclature of Organised Beings adopted by the International Congress of Zoology at Paris, 1889," and confirmed at Moscow, 1892. Dr. Blanchard, 7 Rue des Grandes Augustins, Paris, will send a copy gratis to any professor, director of museum, assistant in museum, or any learned society, on application.

THE *Kew Bulletin* announces that the Queen has ceded to Kew Gardens the tract of land known as the Palace Meadow. This gracious act will allow the public easy access to the arboretum.

DR. FERDINAND LATASTE has several communications in *Actes Soc. Sci. Chili*, iv., part 4, one of which is illustrated by a plate, on the abnormal development of the horns in ruminants, which will be of interest to those who followed Mr. Holding's remarks on the same subject before the Zoological Society on February 5 last.

WE learn that Mr. Sinel has taken charge of an oyster culture establishment in Jersey, and that Mr. Hornell is therefore in entire charge of the Jersey Biological Station at present. The first instalment of the second series of Studies in Marine Zoology is to hand, the high standard of the preparations being fully kept up. The new slides are a colony of *Sphærozoum*, the larva of *Antedon*, and an entire Pteropod *Cresis*. We have also received the *Journal* containing the descriptions and plates; the latter are a great improvement, and leave scarcely anything to be desired. Mr. Hornell announces a new series of microscopical studies in botany at the absurd price of 21s. for 20 slides, with letterpress and descriptions, for which there should be no lack of subscribers.

THE *American Geologist* informs us that the Legislature of Michigan is considering the advisability of entering upon a complete topographical survey of the state, in co-operation with the United States Geological Survey. Maine is also making efforts to found a geological survey of the state.

THE first instalment of the International Geological Map of Europe, which was promoted at the Geological Congress held at Bologna in 1881, has just been issued. It consists of six sheets, and embraces Iceland, a small part of the coast of Greenland, Northern and Central Germany, Belgium, Holland, and portions of Eastern France, Southern Denmark, and Northern Austria. The second part, which will consist of ten sheets, will be issued during the current year, and will contain Great Britain, France, Portugal, Southern Germany, Switzerland, the greater part of Italy, and the western portion of Austria-Hungary. There will be forty-nine sheets altogether, on a scale of 1 : 1,500,000, or nearly twenty-four miles to the inch. Dr. Hauchecorne and Professor Beyrich are superintending the work, while the topographical details are being laid down by Professor H. Kiepert. The subscription price for this map, from Mr. Edward Stanford, is ninety shillings, if subscribed for before June 25, but after this date the price will be raised to six pounds.

THE promoters of the Macleay "Memorial" volume state that it is not a financial success. We are not in the least surprised to hear it, and have already remarked on the inconvenience of this method of publication (NATURAL SCIENCE, vol. iv., p. 242).

THE *Birmingham Post* states that Mr. Cecil Rhodes, as chairman of the British South Africa Company, has offered a tract of land of 200,000 acres in Mashonaland as a game preserve, conditionally on those promoting this preserve-scheme undertaking to provide the funds necessary to enclose, stock, and maintain such an area. It is suggested that a society thus started could hope to become self-supporting and possibly remunerative by the export of animals to menageries and museums. Mr. Selous, Mr. F. J. Jackson, and Mr. J. E. Harting are among those specially interested in the scheme.

BY the kindness of Dr. Henry Woodward we are informed that a letter has been received from Dr. Forsyth Major, dated Central Madagascar, February 22. Dr. Major landed at Mananzari and proceeded to Fianarantsoa, afterwards

journeying to Ambolismatrasua, and making Ivolismanitra and Ambolismanga. In the latter place he met with some opposition from the Governor; but by sending a special courier to the acting British Vice-Consul at Antananarivo he obtained a passport from the Prime Minister, informing the natives that he was a British subject and a naturalist, and has since been treated with the utmost consideration. He has made a detailed exploration of the Tanala country, and has been successful in finding several new mammals and reptiles, and has made an extensive collection of plants. Dr. Major is in good health. He has not had any communications from England, and this letter is the first that has been received from him in this country, although he mentions several others that he has written and despatched.

THE industries of farming and oyster-culture which are pursued in Essex, have caused the Council of that county to make biology an important part of its technical instruction. There is a biological laboratory at Chelmsford, and a useful little monthly, *Biology Notes*, is issued under the direction of the Staff-lecturer, Mr. D. Houston. This gives the programme of work for the current month, the returns for the month past, a Naturalist's Calendar compiled by the students, practical lessons for use in the classes, and articles on applied biology for the benefit of the agriculturist. So successful has the work been that it may possibly give rise to a permanent Agricultural College for East Anglia.

In connection with the experiments on oyster-culture, begun last year at Brightlingsea, with the help of the Colchester Corporation, a small marine biological station has been fitted up. Owing to the weather, which killed the first fall of spat, and to other causes, experiments to determine whether the continental system of rearing oysters on tiles would succeed in the River Colne were hardly conclusive; but as the Fishmonger's Company has subscribed £50 to their continuance, it is hoped that the question will soon be settled. Meanwhile, the expert in charge of the experiments, aided by the staff, has made a good collection of marine animals, which are now being used in successful demonstrations to the local fishermen. Subjects out of the scope of the practical fisherman are also being dealt with; thus Mr. W. M. Webb has recently published, in the *Journal of Malacology*, vol. iv., p. 15, an interesting description of the dimyarian stage passed through by the young oyster. Those in charge of the station hope that it will soon be recognised by London biologists as a place where work may be done, and whence important contributions to science may emanate, to the credit of the Essex County Council.

DR. SCHARFF'S paper on the Irish Caves has already extracted information, for Mr. R. J. Ussher and Mr. J. Coleman contribute to *The Irish Naturalist* for April some notes of interest on the subject.

WE are indebted to the *Pall Mall Gazette* for the following delicious extract taken textually from the *Unione Liberale*, a newspaper published in Perugia: "In case the Dervishes attack us the Admiralty has sent the warship 'Dogali' to the island of Cuba, now the scene of a bloody strife between the Spanish and the English." We commend Perugia to the notice of Professor Guido Cora and his admirers.

CORRESPONDENCE.

A Complaint from the Wilderness.

Some time ago, having occasion to write to the Secretary of the Zoological Society about another matter, I enclosed the description of a new species of Coccidæ, of special interest, which I had just found at that time. In my letter I remarked that it could be printed in the *P.Z.S.*, or, if the Society did not care to print it, I should be glad if he would kindly send it on to the *Annals*.

After a good while, I saw by the papers that the description had been read at the Society, and presumed that it would be published in due course. But to-day I get it back, with no further explanation than the statement that the publication committee do not consider it suitable. The Society had kept the MS. over two and a half months.

Now, I have no complaint to make, of course, about the non-publication of the MS.; indeed, I had some little doubt in my own mind as to the suitability of the *P.Z.S.* for the introduction of a new insect, however excellent it might be for a new monkey. The wording of my letter to the Secretary was the result of this doubt.

The point I want to raise is this. Is it right for a Society to temporarily accept a MS., retain it some months, have it presented at a meeting, and *then* decline to publish it? Is not such a proceeding discourteous to the author, to say the least? Is it not calculated to deter those not among the select few from contributing MSS. to the Society? I do not say that it is intended to have this result; but whatever may be intended, surely such a method of procedure is both objectionable and unnecessary. I am raising the point now in reference to a particular instance, but the same thing has happened to my knowledge on other occasions, and probably happens often. At one of the very first meetings of the Zoological Society which I attended, a paper on fishes was read, which had been sent in by an old and distinguished South American naturalist; it was received with remarks about its being not up-to-date, and unsuitable for publication. The principal speaker was a very learned authority on fishes, and I can well believe that all he said was true; but, all the same, I thought the Society had not acted very courteously to the absent author.

T. D. A. Cockerell.

New Mexico Agricultural Experiment Station (U.S.A.).
February 25, 1895.

[We print Mr. Cockerell's letter because we wish to keep our columns open, so far as is possible, to any scientific man who has a complaint of general interest to make. We have dealt elsewhere with the question of the publication of papers. In the present instance, however, we have no doubt that the Publication Committee, which is elected by the Fellows, was acting only in the interest of the Society.—Editor, Natural Science.]

The Methods of Organic Evolution.

Mr. Galton has recently repeated, in *Mind*, his suggestion that *species* have originated by *discontinuous* variation, *i.e.*, by a series of sudden leaps, or sports, from one position of organic stability to another; and that this method of evolution is independent of the processes of *selection*. This theory, which is based upon Mr. Galton's statistical enquiries into heredity and regression, has been criticised by Mr. Wallace in the *Fortnightly Review* for February and March.

Mr. Galton, strangely enough, appears to overlook the fact that this theory of discontinuous variation between definite positions of organic stability, so far from in any way running counter to the theory of selection, entirely harmonises therewith: for the alleged process, if indeed a *vera causa*, must always be controlled by the master-process of Natural Selection. Clearly, if the—organically stable—characters produced by such discontinuous variation be disadvantageous to the species and ill-adapted to its requirements, they will immediately be swept away by Natural Selection.

This I find—as might be expected—insisted upon by Mr. Wallace in the second instalment of his article; but he strangely ignores the consideration that if such (alleged) variations prove advantageous, the work of Natural Selection is really far shorter and easier than we had hitherto supposed possible. This may be shown by a series of propositions, based on the assumption that Mr. Galton has found a *vera causa*. (1) Such sports, being the expression of definite positions of organic stability, will occur more frequently than mere "chance" variations. (2) They are, therefore, less likely to be swamped by the interbreeding which Darwin recognised as fatal to "chance" sports; and, especially when *particulate*, they are likely to hold their own in at least a proportion of the offspring. (3) Even were all such sports swept away by some disease or catastrophe, yet precisely the same varieties would soon re-occur in another generation, since they are the result of definite positions of organic stability. (4) Since such sports may, in one generation, achieve by a bound a position that could be attained only after many generations by the cumulation of small varieties, then clearly the process of species-making is greatly shortened. (5) It would appear, therefore, that a species may appear more than once, *i.e.*, may reappear even if once extinguished. (6) Obviously all such sports—the result of discontinuous variation—will be, when advantageous, preserved by selection even more certainly than will slight variations.

On the other hand, however, a sudden sport may often prove prejudicial to the species, although the same variation, reached by slow stages, would be beneficial; for the suddenly varying species may outstrip the slowly varying environment. For instance, the protective efficacy of mimicry depends upon the mimicking insect keeping pace with the mimicked; and if the latter—here the "environment"—vary slowly, so must the former: a sport of the mimickers, anticipating by centuries the final coloration of the mimicked, would be fatal. But sports in insects mimicking leaves would probably be beneficial.

F. H. PERRY COSTE.

Ravenshoe, Burnt Ash Hill, S.E.

MINNEAPOLIS AND THE CENTRAL ZOOLOGICAL BUREAU.

RECENT inquiries and suggestions from some of your immediate neighbours impel me to send you these few lines bearing upon your editorial "A Universal Zoological Record," in No. 34, vol. v., of your valued periodical.

The editorial seemed rather unfair to Mr. Barrows, who is printing a card catalogue of some zoological literature under my supervision, and for that reason I desire to give you a few facts in regard to the card catalogue we offered.

For several years we have been printing a card catalogue of the zoological literature available to the students in the Department of Animal Biology of the University of Minnesota. Last year we decided, at the suggestion of Dr. Thos. G. Lee, of this University, to offer to others what we had, and in the event of receiving sufficient encouragement make arrangements for obtaining the titles of all zoological articles.

At this time there was no evidence that Dr. Field's international bureau had any vitality, and we felt the way was clear for Mr. Barrows to issue his circular under date of August 10 without any apologies. To this circular we received encouraging and highly complimentary letters from all parts of our own country and from England. Some, without further questioning, sent in their subscriptions on the rather indefinite and extreme terms of the circular letter. The circular also brought us a kind letter of surprise from Dr. Field. To this we replied promptly,

and asked some definite and pertinent questions. We waited so long in vain for answers to our questions that we felt none were contemplated, presumably because the international bureau was even less than a castle-in-the-air. Accordingly, we began to make arrangements for enlarging our list of periodicals, and were about to issue a second circular guaranteeing at least ninety-five per cent. of all zoological titles at a definite and very low price, when we received a reply to our questions from Dr. Field (who had been as prompt as circumstances would permit).

Dr. Field's letter indicated an actual move towards a realisation of the international bureau; and in the light of the facts disclosed we dropped all our plans, and expressed to Dr. Field our best wishes.

We present no claims for glory, priority, or anything of that sort in this matter. Our only desire is to see this very important work begun. Once begun it will be continued. At present, however, there is altogether too much importance attached to the active co-operation of certain excellent but painfully "slow" societies.

We are still printing our own card catalogue, and are furnishing it to a few friends, but are no longer offering it at any price. We urge everyone to encourage the movement aiming at the establishment of a central bureau at once.

University of Minnesota, Minneapolis, Minn. HENRY F. NACHTRIEB.
March 27, 1895.

THE LEEWARD ISLANDS.

In our March number we praised the colonists of these islands for their practical appreciation of science, especially as applied to agriculture. We regret to hear as follows from Mr. C. A. Barber:—

"By vote of the Local Legislature the office of Superintendent of Agriculture has been abolished, and the Department of Agriculture of the Leeward Islands, as such, no longer exists. The Library which I have been engaged in collecting during the last few years, and to which any journals and reports received have been added, I have handed over to Mr. Watts, the Government Analyst, Antigua, as Secretary of the Antigua Agricultural Society." Mr. Barber's address for the present is 16 Holborn Viaduct, London, E.C.

WANTED.

The following numbers of NATURAL SCIENCE: *No.* 8, *October*, 1892; *No.* 11, *January*, 1893; *No.* 12, *February*, 1893; *No.* 23, *January*, 1894. *The Publishers are prepared to give half-a-crown apiece for clean copies of the above-named numbers until further notice.*

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"NATURAL SCIENCE" *is published on the* 25*th of each month; all advertisements should be in the Publishers' hands not later than the* 20*th.*

Publishers sending Books for Review are particularly requested to take note of our Change of Address.